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ABSTRACT

The first section of this volume includes articles on cross-cultural teaching: "Mau Piailug's Navigation of Hokule'a from Hawaii to Tahiti," by David Lewis; "The New World Order and the Globalization of Social Science: Some Implications for Teaching Cross-Culturally," by Amarjit Singh; "Ponape: Cross-Cultural Contact, Formal Schooling, and Foreign Dominance in Micronesia," by Nat Coletta; "Effects of Motivational and Employment Programs on Scholastic Behavior among Students from Economically Disadvantaged Groups," by Carl LaPointe; "In a Jerusalem Ulpan," by Cynthia Pincus; "Sociolinguistic Competence and Second Language Learning," by Janet Holmes and Dorothy Brown; "Popular Culture in Cross-Cultural Perspective," by Margaret King; "Teaching the Teachers of Hawaiian Children: Training and Consultation Strategies," by Junko Tanaka-Matsumi and Roland G. Tharp; and "Culture and the Role of Client Expectancy in Psychotherapy," by Howard Higginbotham. The second section focuses on investigative strategies in cross-cultural empirical research: "Some Epistemological and Methodological Issues in the Design of Cross-Cultural Research," by B. James Starr and Suzanne Wilson; "Interdisciplinary Collaboration in Cross-Cultural Research," Social Science Research," by Norman Dinges; "Organizing for Cross-Cultural Research," by Eleanor Elequin; "Limitations of Anthropological Field Work," by Anne-Katrin Eckermann; and "Cross-Cultural Comparisons of Anxiety: Methdological Problems," by Sagar Sharma. (CLK)

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Volume 5, 1977

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Edited by B. James Starr

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MAU PIAILUG'S NAVIGATION OF HOKULE'A FROM HAWAII TO TAHITI

DAVID LEWIS

I

Introduction

The 60 foot double canoe *Hokule'a* is a 'performance accurate' replica of an ancient Polynesian voyaging vessel. Her underwater proportions and lines were taken from the underwater sections of the Tahitian and Tongan double canoes that were recorded by Cook and his colleagues and which were found to be identical in the two areas (Cook, 1777; Banks, 1962:364). Her sails were of the Polynesian 'claw' type. Above the waterline, *Hokule'a* is of more recent Hawaiian design and she is constructed of modern materials. However, neither of these latter circumstances would materially affect her performance in terms of seaworthiness, windward ability and speed in the open sea.[1]

After sea trials in Hawaiian waters, a crew of 17 sailed *Hokule'a* from Hawaii to Tahiti in 31 days, arriving on June 4, 1976. Although a 'chase boat' accompanied the canoe for safety and to accommodate cameramen, the 2,500 mile voyage was navigated entirely *without instruments or outside navigational information.*

The navigation was in the hands of the initiated Micronesian navigator Piailug from Satawal atoll in the central Carolines. His assistants were Rodo Williams, an experienced Tahitian ex-schooner skipper, knowledgeable in bird and swell signs of land, and myself. It must be stressed that Williams and I played auxiliary roles only. Piailug was navigator; his were the decisions.

The central question at issue before the voyage was whether Piailug would be able to expand his 'closed' and geographically restricted indigenous navigational system into other and wider reaches of the open Pacific; more specifically, whether his traditional knowledge would be relevant and applicable to the longest sea lane traversed in all Polynesia. His achievement is the answer.

TRAINING AND INITIATION OF A NAVIGATOR IN THE CAROLINES[2]

Piailug

Pius Piailug was born in 1932 so was 44 years old at the time of the voyage. His systematic training as a navigator started at the age of 6, when his father began teaching him every evening after dinner and

---

[1] *Hokule'a* was constructed and her voyage organized by the Polynesian Voyaging Society, Honolulu, as the State of Hawaii's bicentennial project. The grant of fellowships by the East-West Center, Honolulu, made it possible for several of us, including Mau Piailug, Rodo Williams and myself, to take part.

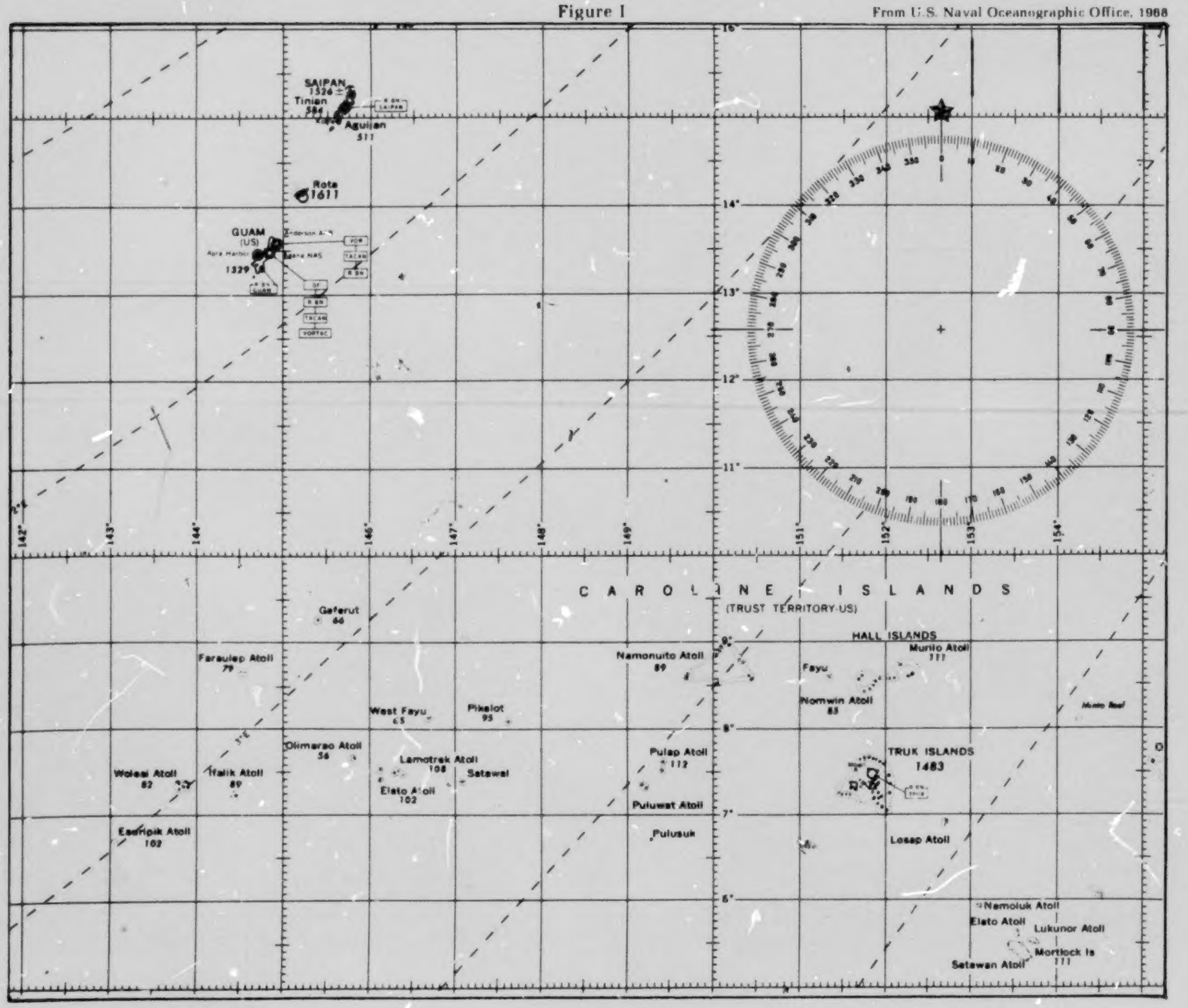
[2] From unpublished field notes, Satawal, 1973. My chief mentors were the elder navigators Namonour and Epeimai.

taking him on canoe voyages to Pigaloe, Puluwat and Lamotrek (see figure 1). Twelve years later, at the age of 18 he was initiated as a *ppalu* 'navigator' (see below). Since then he has voyaged to Truk, Nominuito, Woleai, Ifalik, Pik, Pigaloe, Lamotrek, Puluwat, and Pulusuk.

A voyage of a different order was the round-trip voyage to Saipan in the Marianas. This long journey (475 nautical miles direct, 422 of open sea if made via the uninhabited Pigaloe—the usual procedure) was abandoned about 1904 or 1905 (S. Riesenberg, *pers. comm.*). It was revived in a Western style sailing vessel without charts or instruments by Hipour of the neighboring island of Puluwat (Lewis, 1976: 32-50-5, 107-9, 119, 217-222).

Figure I

From U.S. Naval Oceanographic Office, 1968


SAIPAN
1526
Tinian
584
Aguijan
511
Rota
1611
GUAM
(US)
Anderson AFB
Apra Harbor
Agana NAS
1329
VOR
TACAN
DF
VORTAC
14°
13°
12°
11°
9°
8°
7°
6°
142°
143°
144°
146°
147°
148°
149°
151°
152°
153°
154°
C A R O L I N E I S L A N D S
(TRUST TERRITORY-US)
HALL ISLANDS
Murilo Atoll
111
Fayu
Nomwin Atoll
83
Namonuito Atoll
89
Gaferut
66
Faraulep Atoll
79
West Fayu
65
Pikelot
95
Olimarao Atoll
56
Lamotrek Atoll
108
Satawal
Elato Atoll
102
Woleai Atoll
82
Ifalik Atoll
89
Eauripik Atoll
102
Pulap Atoll
112
Puluwat Atoll
Pulusuk
TRUK ISLANDS
1483
Losap Atoll
Namoluk Atoll
Elato Atoll
Lukunor Atoll
Mortlock Is
111
Satawan Atoll

The following year and several seasons subsequently, the navigators of Satawal, shamed by Hipour's exploit, began making the voyage by sailing canoe again (Lewis, 1975, 32n, 278-80), thus initiating a renaissance of voyaging. Piailug made this voyage in 1974 in atrocious weather, staging at Pigaloe and again at Rota 6 days later. Two more days were needed to reach Saipan. The return was less eventful, the passage from Saipan to Pigaloe taking 7 days.

On three occasions Piailug has been forced to swamp his canoe in typhoons, and he and his crew were once five days in the water. During one of these desperate incidents one man was taken by a shark.

Piailug was married at 16. He has 17 children. He speaks English fairly well but is only marginally literate. Together with Repungelap, who led the first Saipan voyage in modern times and a more recent voyage to Okinawa, Piailug ranks as master navigator on Satawal and perhaps the whole Carolines. He is the last man to have been initiated on Satawal, the procedure being considered pagan now that the island has become Christian.

Initiation

The United States and Britain have but one institute of navigation apiece. Each of the little atolls of Puluwat, Satawal, Pulusuk, Lamotrek and Woleai have two each—the schools of *warieng* and *fanur* (Lewis, 1975, 33, 137, 247n). The teachings of these two schools are practically identical, and instruction in navigation does not differ materially from island to island. In the outline of navigational training which follows I will, therefore, include information from both schools on the islands of Satawal, Puluwat and Pulusuk.

Training of 'navigators' at sea usually begins at around 8 years of age. The cornerstone of Carolinian voyaging is the star 'compass' (considered below) which is simply a rather more systematized version of the Polynesian *kaveinga* (Tongan) or 'star path' (Lewis, 1975: 48-72 *passim*). In addition to the large part of training which is conducted at sea, the boys are taught mainly in the men's house. Pebbles are set out in circles or squares to indicate star points and swells. The star 'compass' is usually represented by a circle of stones; swell patterns by a square with sticks laid across—forming a figure comparable to a Marshallese 'stick chart'. Prodigious feats of memory are demanded of the pupil, for he has to learn by heart the star compass bearings of every known island from every other one. The Puluwat repertoire, for instance, which is virtually the same as the Satawal one, includes sailing directions (of which star courses are but a part) for fifty commonly made journeys and as many more that have now been abandoned (Gladwin, 1970). Needless to say there is a formidable array of facts to be mastered about sailing days between islands, swells, waves and currents, homing birds and weather prediction by stars.

A variety of mnemonics facilitates the student's task. He may be asked, for example, to reach out with an imaginary *ey-ass*, a hooked breadfruit picking pole, towards a particular star point and "draw back" all the islands that lie in that direction. Then, just as in the Gilberts and Polynesia, stories are woven around the stars, wherein they figure as people. Piailug has a host of such tales and I have also seen him have his pupils re-enact them by arranging them in a circle with each one representing a star point. There are 'star dances' too, and there are chants, many of such grave magical import that voices are lowered whenever the secret names of the seaways are spoken.

Periodically, when enough responsible candidates have reached the required standard, the whole community is mobilized and an 'initiation' (*poa*) is organized.[3] Initiation is in effect a concentrated 4-6 months cram course. Preparations for an 'initiation' are made well in advance—bowls are carved, adzes relashed, *lava lavas* woven on back strap looms; breadfruit and taro are gathered.

Initiates are taken to the men's house where they will live. No water may pass their lips, nor may they wash in water. Coconuts must provide their only drink. They must remain celibate throughout, just as when building a canoe or prior to a voyage. Breadfruit and taro are cooked separately be men and women and then placed in a great consecrated mahogany bowl. The head navigator intones a prayer. The 'waistcloths' are next distributed and the food apportioned, one part of the initiated, one for *fanur* and another for *warieng* (the two navigation schools).

The people depart and the head navigator discourses on the secrets of navigation for the rest of the night and all the next day. He is followed through the weeks to come by relays of navigators who continue the intensive instruction. Each stage, usually of 3-4 weeks duration, is initiated by the drinking of a special potion, and after the fifth, *safei neiror*, has been drunk the land part of the training is finished. The youths can now go home at night but must still continue celibacy. From then on initiation continues on the ocean, for the sacred sea charts may only be learnt on the water.

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[3] In describing *poa* (initiation), in contrast to navigational instruction, which still flourishes, we are unfortunately using the 'ethnographic' present.

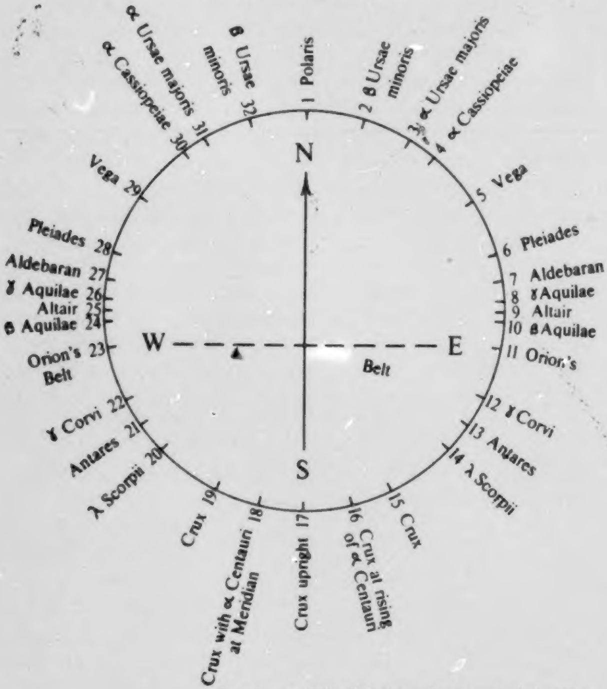
The young men are now *pplau*—'navigators.' The sacred chants will protect them against sharks and storms. Any who is judged experienced enough may now take charge of a deep sea canoe.

THE CAROLINIAN NAVIGATION SYSTEM

The Star 'Compass'

Carolinian navigation centres around the star compass, which is conceived of as made up of points on the horizon precisely indicated by the rise and set of certain stars. Any one of these stars or constellations is only in its 'indicator' position for a very short time. For six months of the year, it is above the horizon in daylight and therefore invisible, but the horizon point named after it remains immutable.

Figure II
Carolinan star compass


N
S
W
E
1 Polaris
2 β Ursae minoris
3 α Ursae majoris
4 α Cassiopeiae
5 Vega
6 Pleiades
7 Aldebaran
8 γ Aquilae
9 Altair
10 β Aquilae
11 Orion's Belt
12 γ Corvi
13 Antares
14 λ Scorpii
15 Crux
16 Crux at rising of α Centauri
17 Crux upright
18 Crux with α Centauri at Meridian
19 Crux
20 λ Scorpii
21 Antares
22 γ Corvi
23 Orion's Belt
24 β Aquilae
25 Altair
26 γ Aquilae
27 Aldebaran
28 Pleiades
29 Vega
30 α Cassiopeiae
31 α Ursae majoris
32 β Ursae minoris

From Lewis, 1972, adapted from Goodenough, W.H., 1953: *Native Astronomy in the Central Carolines.*

In most islands, including Satawal, 32 points are named, though others seem to have been added in Sonsorol, Pulo Ana and Merir (Goodenough 1953:5). The actual number of indicator stars making up the compass is a good deal less than 32, because most stars mark two positions, one at rise in the east and the other at set in the west. On Puluwat, Satawal's neighboring island and voyaging rival, the prefix *daane* denotes rising or east and *doloni*, setting or west. The Pole Star, known as 'the star that does not move', marks one position only. The Southern Cross, on the other hand, indicates no less than five—at rise, half risen towards the upright position, upright, half way down, setting.

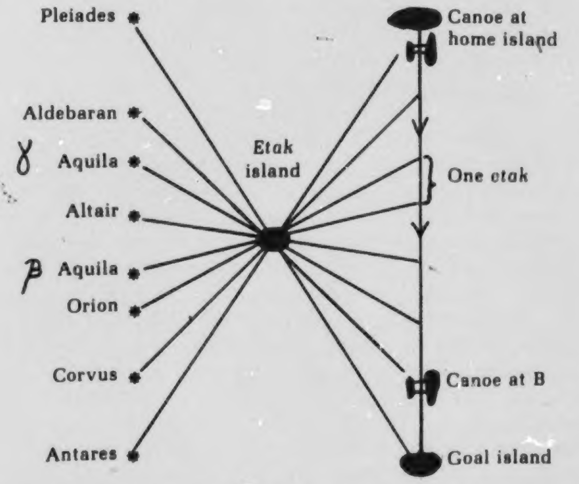
It can be seen from the figure that the star points are irregularly spaced. This makes impossible any simple comparison with the evenly spaced points of a magnetic rose, a circumstance that must largely account for the star compass' survival.

Altair is taken by the Carolinians as indicating east and west, despite the fact that the star actually rises and sets 8½° north of the east-west line (Lewis, 1975:64-5). This Carolinian concept, erroneous in terms of our own astronomy has, as we shall see, a significant Indian Ocean parallel.

Etak

The *etak* (Central Carolinian) method of visualizing one's position in relation to islands in the ocean is the only significant Pacific navigational concept that is apparently confined to one archipelago—the Carolines. The best description of *etak* has been given by Gladwin (1970: 181-195). What follows is a somewhat sketchy outline.

An *etak* island is a reference island, far out of sight but visualized as 'moving' as a voyage progresses, from one star position to another.


Pleiades
Aldebaran
γ
Aquila
Altair
β
Aquila
Orion
Corvus
Antares
Etak island
Canoe at home island
One etak
Canoe at B
Goal island

(The distance between the home and goal islands is here about 100 miles)

When the canoe in the illustration is leaving its home island, the *etak* island lies in the direction (the term used is 'under') Antares. By the time the canoe has reached B, the *etak* island will have 'moved' to the star point Aldebaran. When it reaches the goal island, the *etak* island will be 'under' the Pleiades.

As the Carolinians picture it, the canoe is standing still and so are the stars. The home island 'moves' further and further behind as the voyage progresses and the goal comes closer and closer. All the while the navigator is estimating in his mind how far back the *etak* island has 'moved'. Each star point that it 'moves' back corresponds to one *etak* made good on course.

Etak supplies no new information to the navigator. It is simply a method of sorting out and storing in his mind the data he already has. He pictures the progress of the voyage rather as if he were a passenger in a train. His own railway compartment seems to be motionless to him; but outside the window the telegraph poles appear to be rushing towards the rear of the train, against a backdrop of immobile distant hills. The compartment is the canoe, the telegraph poles are *etak* islands and the hills are stars. It goes without saying that the Carolinians are perfectly aware that the islands actually remain stationary.

An *etak* is a variable distance. This is so partly because the star points themselves are unevenly spaced and partly because the distance away of the *etak* island varies; nor, in practice, does it usually form an equal-sided triangle with the other two islands. However, the *etak* island is generally so chosen as to make an *etak* segment somewhere around 20 miles.[4] The first and last two *etak* of a voyage are exceptions. These are the '*etak* of sighting' and the '*etak* of birds', and both are absolute distances of 10 miles.[5]

Latitude by Eye

Now the height above the horizon of the Pole Star (or substitute stars in the days before Polaris was where it is now) equals one's latitude. For instance, the North Star at Satawal and Puluwat stands at 7½°, and at Saipan at 15°, so the latitude of Satawal and Puluwat will be 7½°N., and of Saipan 15°N.

In Satawal the height of the Pole Star is judged by eye or by the span of the fingers loosely extended at arm's length. This measure is one *ey-ass* or 15°. The senior Satawal navigators Namonour and Epeimai (1973, pers. comm.) told me that the North Star was half an *ey-ass* above the horizon at their home island and one *ey-ass* at Saipan, proportions that are exactly right. Piailug in Honolulu, using his extended hand, put the height of Polaris at "nearly one and a half *ey-ass*." One and a half *ey-ass* is 22½°, the latitude of Honolulu is 21°.15°, so Piailug's estimate could hardly have been more accurate.

The other method of finding latitude without instruments, and the only one available south of the equator where the Pole Star is hidden, is by overhead or zenith star. This depends on the fact that each star crosses the heavens along a particular line of latitude. The star's declination (celestial latitude) equals the latitude of all places it passes over on the earth beneath. Its course is the same night after night, though it does alter over the centuries. If the star Spica, for example, whose declination is 11° S, is observed to mount to the zenith of its arc directly above one's masthead then, if the mast be vertical, one is 11° south latitude.

Zenith stars are mentioned here because their use in a Carolinian navigator's repertoire was reported a century ago. Captain Sanchez y Zayas interviewed the navigator of a Carolinian canoe that had arrived on Tinian from Elato 450 miles to the south. In a passage, tantalizing for its brevity, Sanchez wrote: 'they fill a cane with water and observe the stars in the zenith, and thence study the position of the vessel' (Sanchez, 1866:263).

Present day Carolinian navigators, however, have lost all knowledge of this technique (Piailug, Hipour, *et al:* pers. comm. 1969-1976), unlike their Polynesian counterparts, especially in Tonga and Tikopia, who have retained some memory of it (Lewis, 1975: 235-8).

A FORMER INDO-PACIFIC SYSTEM OF NAVIGATION

The navigation that is practiced in the Carolines is the most complete non-instrumental discipline extant. But not a little lore still survives in other parts of the Pacific, and analogous techniques were recorded from the Indian Ocean in earlier times. This suggests that Carolinian navigation is but a special case within a wider system. A brief discussion of the evidence for a one-time ocean-wide system or systems is desirable if we are meaningfully to assess the significance of Piailug's extension of his concepts and methods to the wider ocean.

Apart from *etak*, which so far has not been reported outside the Carolines, every significant navigational concept and technique is found to be, or has been, virtually Pacific-wide in its distribution. The Polynesian-Micronesian cultural distinction seems particularly irrelevant in this respect, differences between archipelagos being largely functions of local geography (Lewis, 1975:11, *et seq.*; but Akerblom 1968 dissents from this view). For example, that cornerstone of the Carolinian system, the star compass, is merely an elaboration of simple star path steering and functions exactly the same at sea. It is just more economical in the number of stars that have to be memorized. There is Polynesian evidence from Tonga suggesting that a star compass was once used there as well. The 88 year-old Tongan, Kaho, of the Tuita navigator clan, listed eight stars that indicated *directions* rather than the positions of islands. These 'compass' stars, together with others that he had forgotten, had been pointed out to him by his father, a navigator trained in the double canoe era (Lewis, 1975:77).

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[4] The Marquesas, the only available *etak* on the Hokule'a voyage satisfied none of the desiderata.

[5] It should be mentioned that the doubts expressed by Lewis (1975: 243) as to this method being used in the Carolines were based on incomplete 1969 data.

The star compass also shows some of the most striking Indo-Pacific parallels. In 1553 a Turkish admiral called Sidi Ali ibn Husain compiled from Arab sources going back to the 9th century A.D. a work called *Muhit* (The Ocean). The magnetic compass had long been in use by Sīdi Alī's day but certain passages in the *Muhīt* clearly show that it was preceded in the Indian Ocean by a star compass (Von Hammer, 1834). In fact, a compass card marked in star points is illustrated in an Arab book on navigation, the *Majid Kitāb* (Prinsep, 1838). The same star names occur in the east and west just as in the Carolines. A prefix meaning 'rising place' or east *mutalā-Arabic*), and another meaning 'setting place' or west (*majīb-Arabic*) are used. Again like in the Carolines, the Arab star points are irregularly spaced and do not correspond with the magnetic rose. More significant still, the *Muhīt* makes exactly the same 'error' as the Carolinians in taking Altair as their east-west line. The Pole Star was not in its present position when the *Muhīt* was written and many of the southern stars of the Arab compass cannot be identified any more as their names are archaic Indian and Persian. Nevertheless, no fewer than 18 out of the 32 star points appear to be identical in the Indian Ocean and Pacific systems.

The Carolinians, as we saw, estimate the height of the Pole Star and thus their latitude in *ey-ass*. The height of the Pole Star substitutes (Polaris itself was not where it is now) was estimated by the Arabs in finger-breadths, at arm's length, *isbah*, and in identical finger-breadths, *chih*, by the Chinese. An *isbah* and a *chih* both equal about 1½° in our measurement.

Long Asian sea routes were confined to the Northern Hemisphere. Since the Carolines are north of the equator, it is natural that estimates of the Pole Star's height at different islands should be retained in the navigators' repertoire. But the position was different south of the equator where, as we have seen, the North Star is no longer visible. Another method had to be devised for the South Pacific and the zenith star fitted the bill very well (Lewis, 1975: 233-246 but contra Akerblom 1968).

A reasonable assumption would be that latitude by Polaris or its substitutes was invented on the long east-west sea routes that are characteristic of the Indian Ocean, and thence spread, perhaps via the North China Sea, into the North Pacific. Alternatively, the technique may have originated with the Chinese. The zenith star method, on the other hand, we almost certainly owe to the Polynesians, the outstanding seafarers of the Southern Hemisphere. In fairness, it should be mentioned that the *Muhīt* does refer to 'a circle of height' which appears to be the zenith. But I know of no evidence that the Arabs or their Indian and Persian predecessors ever used the zenith star for latitude.

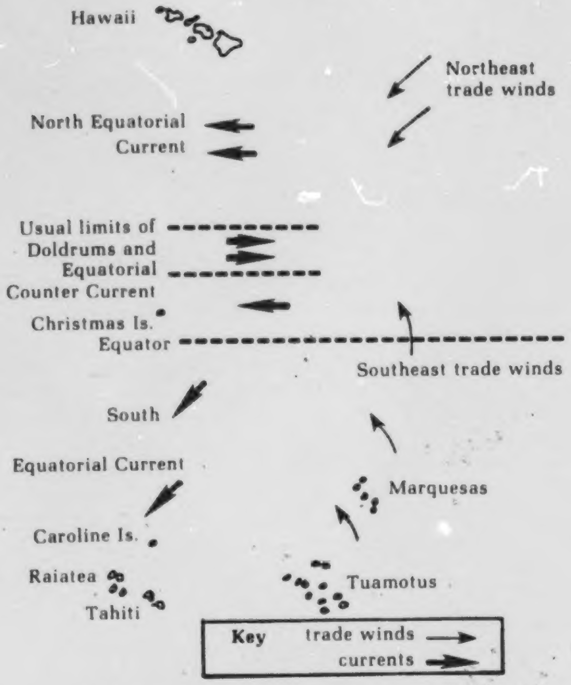
II
THE HOKULE'A VOYAGE

Background

A number of voyages in both directions between the Society Islands and Hawaii are thought to have been made over a 200 year span in the Middle Ages, though the initial colonisation of Hawaii is believed to have been by one-way voyage or voyages from the Marquesas, and to have antedated Tahitian contact by several centuries (Emory, 1963; Emory and Sinoto, 1965; Green, 1966). Fornander gives numerous Hawaiian legends of two-way contact with Tahiti (1880:6-58), while Finney refers to 'archaeological, linguistic and traditional evidence indicating a prolonged relationship between these two areas (Hawaii and Tahiti) occurring between the 12th and 14th centuries A.D.' (1967:152). There is also evidence of a rather different order that I happened upon while studying petroglyphs of sails

The Seaway

The 2,500 mile route between Hawaii and Tahiti lies across the ocean currents and the trade winds, so that it is hardly possible for a canoe ever to have drifted either way between the archipelagos. A computer simulation of drifts and navigated voyages has confirmed, within the limits of a very carefully constructed model, the zero probability of random drifts the length of this seaway (Levison, Ward and Webb, 1972). On the other hand, it is in precisely this cross-wind direction that a curious Tahitian adventurer would choose to probe, so as to penetrate far into the unknown while, at the same time, being sure of a fair wind to speed his return.



(Map, Hawaii-Tahiti, winds and currents)

From north to south, the wind system and currents are: the northeast trades, which generally blow from east-northeast, and are responsible for the west-flowing equatorial current. The doldrums come next, sandwiched between the two trade wind systems, and are characterised by calms and light variable winds, and by the east-flowing equatorial countercurrent. Their usual provenance is between 8° N. and 4° N. The southeast trades hold sway from the southern limit of the doldrums to beyond Tahiti, and are associated with another west-flowing stream, the south equatorial current. Near the equator the S.E. trades blow from the southeast; further south, more generally from east-southeast.

Pacific Island navigators commonly make the valid assumption that oceanic currents follow the prevailing winds (Lewis, 1975:110-115). Thus it is general knowledge that the currents of the tropical Pacific are usually west-flowing, driven before the trade winds. The one important exception is the east-flowing equatorial countercurrent, which is the only stream whose general direction cannot readily be deduced from the wind pattern. In practice, however, this current's only effect is to give a canoe a welcome boost to the windward.

Landfall by sight on the lofty Hawaiian islands allows a margin of error of 10° from Tahiti, and a conservative 'expansion', taking in the line of islands, reefs, shoals, bird zones and swell phenomena that extends as a screen at least to Lisianski Island, provides an 'expanded' arc of 20°. The sight ranges and bird zones of the Society and Tuamotu archipelagos very nearly overlap when approached from the north. Their combined 'expanded' arc is 17°. Such large targets are exceptional on well documented Polynesian and Micronesian regular sea routes (Lewis, 1975: 152-232).

The courses in both directions are roughly north-south, but Tahiti is 250 miles east (up-wind) of Hawaii, so a south-bound vessel must keep closer on the wind and take advantage of every opportunity to make easting. The Hawaii-Tahiti voyage was one of the longest, if not the longest, ever made with any regularity in Oceania, but it is not particularly difficult navigationally. The wind and current patterns favour two-way voyaging and are much the same as those on the Carolines-Marianas route. The Hawaii-Tahiti targets are much the better.

It is not always realized that navigational accuracy is not a function of length of voyage since day to day dead reckoning errors tend to cancel each other out. For example, on our *Rehu Moana* voyage without instruments from Rarotonga to New Zealand in 1965, my daily estimates of distance sailed were out by as much as 40 miles for a single 24 hour period, yet the total error for the whole 1,600 mile passage was only 77 miles (Lewis, 1967:283).

Expanding Piailug's Geographical Terms of Reference

A medieval Tahitian or Hawaiian navigator would have possessed information about the Hawaii-Tahiti seaway exactly comparable to Piailug's about his own and neighboring archipelagos (see chart). The data would have been accumulated on past voyages and would by analogy include distance in terms of an average day's sailing; direction in star path or star compass terms, allowing for mean current set and for a variety of wind conditions; wind and current patterns (with the possible exception of the equatorial countercurrent).

The task of providing Piailug with data of this order fell largely to me.

These data comprised the distance to Tahiti (5 times the Satawal-Saipan voyage), the Tuamotus and Marquesas; the bearings, in star compass points, of Tahiti, the Tuamotus and the Marquesas; the current and wind patterns; the relative heights of the Big Dipper and Southern Cross near Tahiti and the place of disappearance of the Pole Star—the equator. This latter was either familiar to Piailug already or else he grasped the concept of the equator without any difficulty—I am not sure which.

Once this background was filled in, Piailug, using his own familiar navigational concepts described above, was in complete control of the situation. He confidently laid down his strategy for the voyage; the *etak* (the Marquesas) and the star courses to be followed through the N.E. and S.E. Trades (see 'strategy' below).

Piailug explained that, in Carolinian navigation, distances were customarily expressed in 24 hours' run by a sailing canoe under various conditions of wind and weather. The younger navigators, however, had recently begun to use miles as a second system of measurement.[6] He, himself, had been taught the traditional way, but he also sometimes used miles. A day's sailing might be anything from 60 to 100 miles; in *Hokule'a's* case the distance would be further, perhaps 120-140 miles when sailing on a close reach (a little off the wind), as we would be doing practically all the way.

Through the courtesy of the B.P. Bishop Museum, the facilities of the planetarium were made available to us. Louis Valier, lecturer and teacher of celestial navigation, arranged a number of demonstrations illustrating the night sky over Tahiti, and so on.[7] These demonstrations, that cost much of Louis' spare time, were largely wasted because of an error of judgment on my part. I should have insisted that the sessions be restricted, at any rate initially, to Piailug and myself. A meaningful dialogue would then have been possible, making up for the ambiguities inherent in the navigator's limited command of English, by direct reference to specific stars and constellations at any desired date, time of night or latitude. I could have determined, for instance, the precise bearing in degrees of every Satawal star point. This is not so simple a matter as it sounds because some points are marked by large constellations and others by stars that rise and set at an acute angle to the horizon, so that their bearing alters very rapidly. The *ppalu*, 'navigators', of course, know the exact position on the horizon of every star 'compass' point, but they are not accustomed to expressing them in degrees. Piailug, it is true, did sometimes use degrees, but his mastery of the Western system was incomplete, so that his attempts to match star points to degrees were not always successful.

Unfortunately, I gave in to pressing requests to make the sessions 'educational' for the crew and others. Thus a good deal of time was spent demonstrating the night's sequence of steering stars on our proposed departure date, which was beyond the comprehension of the audience, while boring to Piailug. After several visits the *ppalu*, 'navigator', lost interest. He had by then extended his geographical range and had worked out all his star courses; there was not much he could have learned from the planetarium, but the information he could have imparted would have rendered this record more precise.

More fruitful educationally were Piailug's lessons for the crew under the night sky. He pointed out the compass stars and constellations that were most relevant to the coming voyage and taught their

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[6] The 'miles' referred to in this paper are nautical miles. One nautical mile equals approximately 1.15 statute miles.

[7] With an immense amount of labor, Louis Valier extracted from the nautical almanac and tabulated information about steering stars and zenith stars (azimuths, times of meridian passage, etc.) that enabled me to understand and document Piailug's navigation.

Satawal names. He made little stone circles to illustrate the star compass and squares of sticks to show swell patterns.

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On occasion he arranged his students in a circle, assigning to each one the name of a particular star compass point. He told me modestly that he was "teaching not much—very little." But it is indicative of the efficacy of his traditional methods of instruction that some of the crew, who were all beginners, did succeed in memorizing several star points.

There was one complication in 'expanding' Piailug's traditional range that would not have faced the old-time navigators, since it arose solely from the unavoidable necessity of translating star compass points into degrees and then laying them off on a Mercator projection chart. The azimuths (bearings) of most stars vary somewhat with latitude. *Within* the Carolinian system (and equally with ancient Tahiti-Hawaii star courses) this circumstance presents no problem at all, because the stellar sailing directions of all non-instrumental systems refer to *mean* star bearings.

What then was the potential error introduced by the alien intrusion of degrees into the Pacific system? Not all star bearings alter to the same extent with change of latitude. Piailug's chosen steering star through the northeast trades was *Tumur* (Antares), whose declination is 26°. Its change of bearing between Hawaii and the equator is under 3°. The Southern Cross, which was to be our guide for the rest of the way, alters even less. It can be seen that the discrepancy was not a major one.

Data on wind and current regimes was obtained from pilot charts, yachtsmen's logs,[a] observations specially made and recorded for us by Jean Rondiex, captain of the French oceanographic vessel *Coriolis*, from a paper on trade winds and currents (Wyrtki, 1974) and other sources. It became apparent that currents setting westward 20-30 miles a day were to be anticipated for most of the passage. To compensate for this westerly set, as well as for leeway (wind drift), we must deviate to the east of the direct track to the tune of 600-800 miles (see 'strategy'). I duly passed on this information to Piailug before and during the voyage, my estimates being agreed by the Tahitian assistant navigator, Rodo Williams.

It may well be asked how an ancient navigator would have fared with these currents. In fact the very first successful return voyage would have supplied the basic data—the star courses actually steered and the displacement from that course as shown by the place where the vessel finally ended up. The course that would compensate for mean current set and leeway could then readily be worked out. Further voyages would refine the sailing directions as the limits of variability of the currents became apparent. This is not altogether a hypothetical reconstruction, for it is in precisely such terms of particular star paths to be sailed under different conditions of current and wind that star sailing directions are couched in such diverse parts of Oceania as the Carolines, Santa Cruz Reef Islands, Tonga and the Gilberts (Lewis, 1975: 52, 48n, 60).

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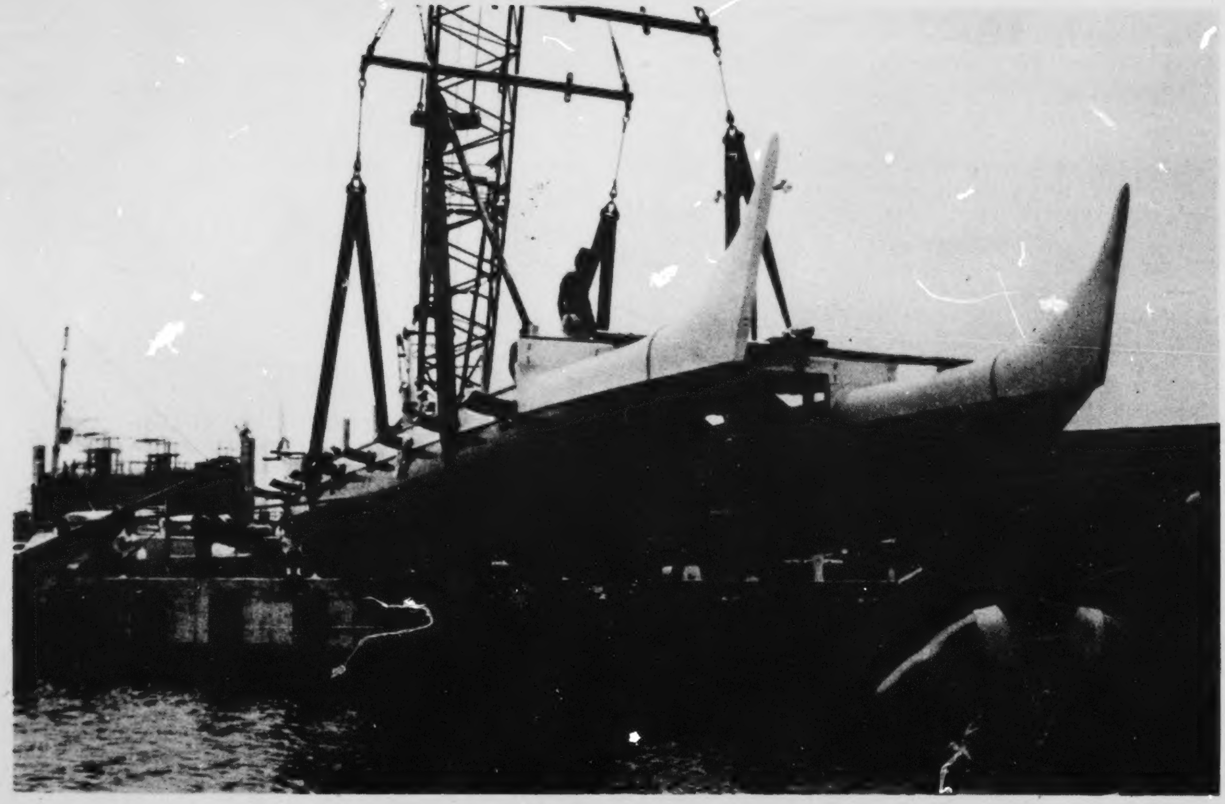
[a] Curtis Ashford, Jerry Williams, Earl Schenck, Louis Valier, Rodo Williams; oceanographic data from Dr. E.D. Stroup.



THE CANOE

As mentioned in the introduction, *Hokule'a's* underwater lines were taken from 18th century Tahitian and Tongan voyaging canoes. Like them, she is essentially a sailing vessel, being too highsided for efficient paddling. There is not the slightest evidence that paddling ever played any part at all in Pacific deep sea voyaging. Indeed, the antiquity of the proto-Austronesian word for sail, *laya(r)*, suggests that, when man first ventured far out onto the Pacific around 1,500 B.C., his vessels were not only sail-powered but had been so for 2,000 years. The first Polynesians to sight Hawaii had something like 4,000 years of sail behind them (Pawley and Green, 1973: 34, 53, 54; Lewis, 1977, in print).

The fact that *Hokule'a* was built of modern materials was also referred to in the introduction, but this circumstance has caused so much confusion that I will enlarge upon it here. Rather strict criteria for the construction of replicas in nautical archaeology have been suggested by McGrail (1975: 3-8). But these have nothing at all to do with '*performance equivalent*' replicas such as *Hokule'a*. She was built to duplicate the *performance*—average speed, weatherliness, leeway—of ancient voyaging canoes with idential underwater lines, rigs and equivalent displacement. She fulfilled this role admirably, her mode of construction being immaterial to the task.

It was not intended that *Hokule'a* provide data other than this. Only a 'construction identical' replica, prohibitively expensive and bristling with problems of lost technology, could have given information about such things as the strength and durability of traditional materials, the efficacy of sennit lashings or the waterproofing of seams. Such was not the object of the *Hokule'a* exercise.

Incidentally, there is a common misconception that is worth correcting here. This is that Tahitian and Tongan voyaging canoes, upon which *Hokule'a* is modelled, were dugouts like the specialized coastal craft of classical Hawaii. They were, in fact, sewn-plank vessels with inserted frames, as indeed, the typical sea-going craft of the 'proto-Polynesians' seem to have been (Haddon and Hornell, vol. 1:130-9, 268; vol. 2:40-1).

Hokule'a's speed and windward ability at sea were not only germane to assessing the likely limitations of prehistoric voyages, they were important factors in Piailug's strategy, and even more vital to his dead reckoning once the voyage itself was under way. Unfortunately, there were only two days when we had the opportunity to measure speed and course made good in relation to the wind by instruments—albeit rather inaccurate ones. For the rest, we had to fall back on informed seamen's estimates.[9]

Our main concern was with the canoe's windward performance, because the whole course to Tahiti would be a windward one. Canoes in Oceania lack deep keels, and are invariably sailed a little off the wind, on a close reach rather than close-hauled. They are kept moving briskly; speed is not allowed to fall off unduly and leeway is thus kept to a minimum. This apparently universal handling technique tends to mask differences in the potential weatherliness of various hull forms and rigs. The hyper-efficient sailing craft of the Gilberts, Carolines and Ninigo, the round-bottomed paddling-sailing canoes of Hawaii, Rarotongan and Papuan outriggers, all make good somewhere between 70° and 80° from the true wind (Doran, 1970 and 1972; Lewis, 1975: 269-70, 269n).

It came as no surprise, therefore, to find that *Hokule'a* when windward, was most efficient on a close reach; speed was kept up and leeway kept down to 10°. In calm to moderate open sea conditions she made good 70° from the true wind; in rough seas the figure was nearer 75°, the leeway angle then being 15°. In light trade winds her speed full and bye averaged 4.5 knots; in strong winds, 6 knots. On the wind she rarely exceeded this speed except momentarily, but, off the wind, she was capable of 8 and, on occasion, 9 knots.

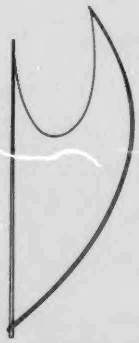
In the matter of speed *Hokule'a* did not compare too favourably with an old-time Tongan double canoe whose speed was measured by Cook. He trailed a patent log behind what appears to have been a *tongiaki* and recorded a mean speed of 7 knots close-hauled, which he thought would be a reasonable average for 'such breezes as generally blow in their sea' (Beaglehole, 1967: 164). Tahitian 'Paheas' *(pahi)*, according to Cook, could sail much faster than his ship, and he gave it as his opinion that they could 'with ease sail 40 Leagues (120 miles) a day or more' (Beaglehole, 1955: 157).

Hokule'a's average daily runs of 120-130 miles seem to fall into the lower range of the ancient craft upon which she was modelled, probably because she was undercanvassed—in a 30-55 knot squall with full sail, the sheets did not have to be eased. *Tongiaki* 'Tongan double canoes' were fitted with balance spars, upon which the crew are believed to have 'sat them out' in strong winds. This suggests that they carried more sail than their modern descendant. Tahitian *pahi*, on the other hand, do not seem to have been so equipped (Haddon and Hornell, 1975, vol. 1. 114-5, 126-141).

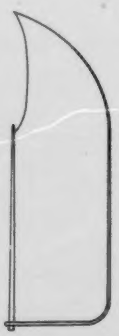
The Sails

Cloth sails were used for the *Hokule'a* voyage to Tahiti, mainly because of absence of command initiative, since a set of pandanus mat sails was actually aboard. This was a pity because it is an open question whether their semi-rigid aerofoil section would not be more efficient than cloth on a canoe stable enough to take the weight aloft of rain-soaked mat sails. Certainly the Ninigo islanders, who use both sorts of sails, carry the canvas ones for everyday fishing, while reserving the matting sails for racing (Lewis, 1975: 263).

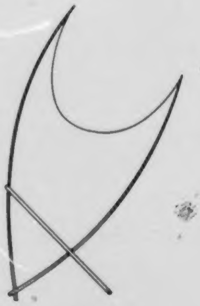
The material of the sails was, however, a good deal less significant than their shape. The Hawaiian sail was claw-shaped (Haddon and Hornell, 1975, vol. 1:6, 7, 25), as was that of the Marquesas (*ibid:* 40). The Tahitian sail, which I have christened a 'half-claw', though easily distinguishable in outline, was 'strictly comparable with the old Hawaiian sail and the two must be classed as variants of the same type' (*ibid:* 122).



HAWAIIAN
CLAW SAIL



TAHITIAN
HALF-CLAW SAIL



TAUMAKO
CLAW SAIL

The claw shape, which, may at first sight appear fanciful, reveals itself upon analysis to be highly functional. The tapering off of the sail towards the claw tips ensures that the sail area to be supported by the marginal spars should decrease away from the point of attachment of those spars. This allows of efficient operation with far lighter spars and masting, less elaborate staying and, in general, much less weight and stress aloft than any other inverted triangular shape would impose. The claw sail was used along the eastern margin of Eastern Polynesia (except the Tuamotus). It reappears, with different masting arrangements, at the extreme *west* of the Polynesian range—in the Polynesian Outliers, Taumako and the Santa Cruz Reef Islands, and probably in Pentecost island in the New Hebrides (Haddon and Hornell, 1975, vol. 2:49, 34). Further west still it was used on Motu *lakatoi* and Mailu *orou* (ibid: 229, 236). The Mailu and Motu are East-Austronesian speaking peoples of coastal southeastern Papua, whose settlements are believed to have been established about the time of Christ (Bellwood, 1975: 13; Golson, 1972a: 19). It is perhaps significant that this same region is one of the last strongholds of that other characteristically East Austronesian artifact, the double canoe (Haddon and Hornell, 1975, vol. 2: 227, 231-40).[10]

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[9] Sea trials were so severely restricted largely because an earlier swamping had revealed the necessity of making a number of time-consuming modifications.

Between the eastern and western claw sail domains, the mid-Eastern and Western Polynesian archipelagos were dominated by two sail types: small vertical inverted triangular sails, some of which might have been simplified derivatives of claw sails; boomed lateens of the simple Polynesian and more sophisticated Indonesian-Micronesian varieties (Haddon and Hornell, 1975, vol. 1, relevant sections).

The unique 'engineering' of the claw sail suggests that, contrary to Haddon and Hornell's classification, it should be regarded as a distinct type. Its distribution would then fit in with the known facts of Polynesian expansion, notably the dispersal west and east from the Samoan region during the last centuries B.C. and the early Christian era (Golson, 1972b: 11). The lateen of Western Polynesia would seem to represent a Micronesian intrusion. The persistence of the claw sail at the margins of Polynesian expansion and its absence anywhere else in the world lend support for its having been a true Polynesian or East Austronesian invention (Lewis, 1977).

Experience with *Hokule'a*'s claw sails soon demonstrated their remarkable efficiency. At first she had been fitted with apex-down sails devoid of any upper convexity; they set poorly and the spars were several times broken. The claw-shaped sails that were substituted for them proved to be very effective to windward and not a single spar broke in the course of 3,000 miles of voyaging.

To arrive at the authentic Hawaiian shape when making these sails was not easy. Models were studied, and Webber's original sketches, made during Cook's third expedition, were examined.[11] The sketches showed sails filled with wind so that their curves were exaggerated. We turned then to sails depicted in Hawaiian petroglyphs, of which a number of tracings were made. These were excellent guides that appeared to have been drawn pretty well to scale.

It was Piailug who selected, cut down, shaped and scarfed the flexible *hau* wood spars to which the sails were laced. In this, as in every other skill, from using an adze with uncanny precision to lashing the cross beams of the canoe, he showed himself a master craftsman.

We referred under 'background' to accidentally coming across evidence of former Tahitian contact with Hawaii when we were studying the petroglyphs. Among innumerable petroglyphs of Hawaiian claw sails photographed by Dr. Emory, three from the island of Maui were seen to be quite different—they clearly showed Tahitian half-claw sails. Dr. Emory confirmed the early date of these petroglyphs, thus ruling out the chance of their having been executed by a Hawaiian sailor returned from Tahiti on a Western vessel. We are left with the only alternative: that a prehistoric artist was depicting the sails of Tahitian canoes that had voyaged to Hawaii in pre-European times at a period when the Tahitian sail had evolved its own specific features.

Steering Technique

The method evolved on *Hokule'a* through sail and error turned out to be one of the several techniques that was widely practiced throughout the Austronesian speakers' vast Indo-Pacific range.[12] Instead of being moved sideways the steering paddle was *raised* and *lowered*. When the blade was pushed down into the sea (towards the vessel's stern) it increased the effective draft aft, and wind pressure on the sails caused the craft to pivot down-wind; when the paddle was raised, up wind. Auxiliary paddles and sweeps could be brought into play for such purposes as tacking. It must be stressed that this steering system, relying as it does on the dynamic balance between wind forces on the sail and water pressure on the hull, can only be used under sail.

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[10] Haddon and Hornell, themselves, did not regard the claw sail as a distinct type; they classified the western varieties as 'lateens'; because of their masting (Haddon and Hornell, 1975, vol. 2:50, 235).

[11] By courtesy of Dr. Kenneth Emory of the B.P. Bishop Museum.

[12] Austronesian languages were spoken by most Melanesians, all Polynesians and Micronesians, as well as by coastal and island peoples from Madagascar to Taiwan, including Indonesia and the Philippines (Bellwood, 1975: 11). The immersion steering technique was used in the Pacific, among other places, in Fiji (Fr. Myret, pers. comm.), Ninigo (Lewis, 1969, field notes), and Tahiti (A large Tahitian steering paddle in the Bishop Museum could not, by reason of its short handle, have been used otherwise). A number of Austronesian (and, oddly Peruvian) sailing rafts were steered by daggerboards on this same principle, though not those of Polynesia (Needham, 1971: 393, 551; Ling, 1956: 35, 47; Hornell, 1946: 66-82; Edwards, 1972: 885; Hornell, 1975, vol. 1, appropriate sections).

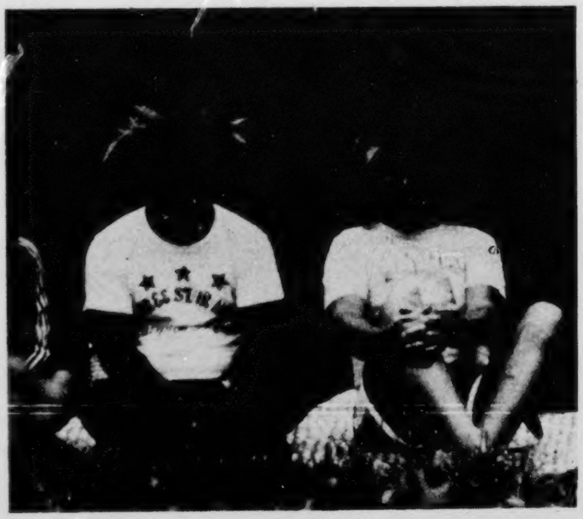
Piailug's Strategy

Hokule'a was to begin her voyage from Honolua Bay on Maui island. Something like three days would be needed to tack north-about round Maui and the 'Big Island' of Hawaii.[13] Once clear of the Big Island, Hawaii, our initial course would be southeast, towards the rising Antares (*Tumur*-Satawalese), in order to make the bulk of the 700 miles or so of easting needed to counteract current and leeway for the whole voyage.[14] This concentration of compensation for current in the first part of a voyage was equally in accord with traditional Carolinian practices and with the seamanlike principle of 'keeping something in hand'. The directions of the Trades likewise dictated that the lion's share of easting should be made north of the equator. The bearing of the *etak* 'island', the Marquesas group, would not alter materially during the first part of the voyage.

The second half of the voyage, through the southeast trades, would begin below the doldrums a little north of the equator (Piailug took the equator as a convenient approximate half way point). The course would be south-southeast towards the position of the Southern Cross half-risen (*Majemehas*-Satawalese). During this second half of the journey, the Marquesan *etak*, originally 'under' Antares, would 'move' north, from star point to star point until, as the Tuamotus came near, the *etak* would bear somewhere between the Pleiades and Vega.

There being no generally applicable way of assessing current strength out of sight of land, Piailug was aiming to make his first landfall in the Tuamotus, up-wind and up-current (east) of Tahiti proper. The precise star courses for the final short leg to Tahiti would depend upon which Tuamotuan island we staged at. This was Rodo Williams' home ground and, in any case, Tahiti was a target that could hardly be missed. Serious star navigation would, for all practical purposes, end with the Tuamotuan landfall.

Piailug would judge our latitude by the height of the Pole Star while we were north of the equator, thereafter, I would use zenith stars. Longitude was quite a different matter; it cannot be determined without instruments so, like all Western seamen before the invention of the chronometer in Captain Cook's day, we must rely upon dead reckoning.



Frank Wandel, International Society of Islands

Piailug's Navigation on the Voyage

On the eve of departure from Maui, Piailug addressed the ship's company through an interpreter. He said:

> "I wish to speak to the whole crew, how they should conduct themselves with reference to food and water and what their behavior should be on the deep sea. I want to tell all of you how you must act on the ocean to survive. On the ocean all the food, all the water, is under the control of the captain. Before we leave throw away all things that are worrying you. On the ocean you will be under the control of the captain and the navigator of the canoe."
>
> "Everything the captain says to do we follow . . . When on the ocean we cannot see any islands; only the things we bring with us help us to survive. We eat together. Everything we do on the ocean is different from on land.

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[13] Various parties, for reasons quite unconnected with seamanship or genuine Hawaiian tradition, tried to divert the canoe south of the Big Island. This would have deprived us of 100-200 miles of precious easting and would have imperilled the success of the voyage.

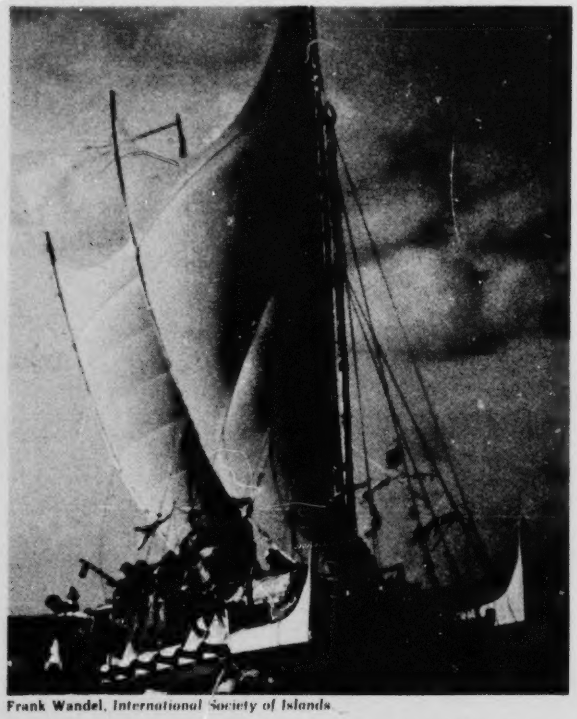
[14] This would be easting *through the water*. If the easterly component of a day's passage was 60 miles, say, and the west-going current set the canoe back 30 miles in the same 24 hours, her actual easting *over the ground* would be 30 miles. We would therefore arrive at the equator about 350 miles east of our starting point, not 700.

> Everyone who wants to ask the captain why we are doing something, ask me first. That is all I have to say; remember all of you and we will see that place we are going to."

Piailug spoke with quiet dignity. His words went unheeded by the crew.

Next day, 1st May 1976, we unbrailed the sails and put to sea around 1500. All times are approximate, since our wristwatches had been left behind or placed in the care of the captain Kawika Kapahulehua as potential navigational instruments. There were 17 men (including two photographers) aboard, plus the poi dog Hoku, the pig Maxwell and a cock and a hen.

The heavy-laden *Hokule'a* pitched into the seas as she came out from under the land and headed north on the starboard tack on the first of the long tacks that would take us past Maui and the Big Island. The northeast trades were variable in direction but generally from east-northeast, force 3 (9-12 knots). The weather was mainly cloudy.



Frank Wandel, International Society of Islands

The 60 foot auxiliary ketch *Meotai*, the 'chase boat', followed the canoe. She, of course, used Western instrumental navigation; after the voyage our daily positions were compared (see track chart). *Meotai*'s crew were, however, very careful not to divulge any navigational information while we were actually at sea.

Piailug took up his station at the stern, a position that he rarely left. He later constructed a hammock of knotted cords strung between two spare steering paddles that he had lashed to the rails. He did not stand regular watches; day and night he remained alert and watchful, intently studying the stars or sun and the swells. When all was quiet he slept, but he slept lightly and, when he was awake, his vigilance never abated.

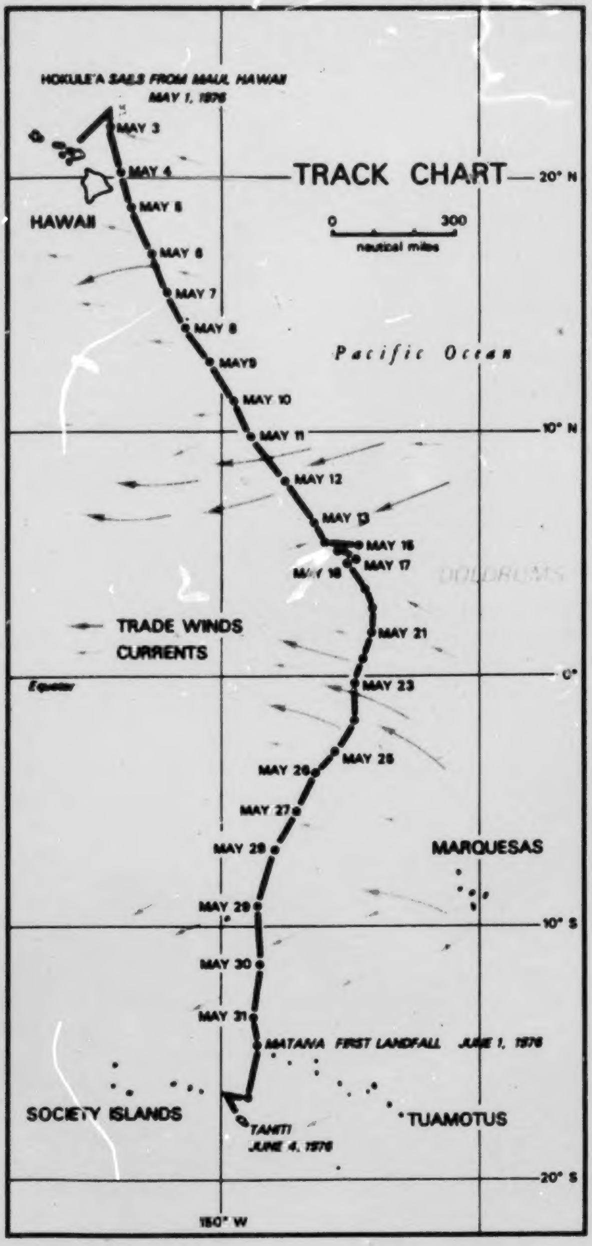
At Piailug's direction, we came about at midnight when we were roughly 30 miles offshore, and tacked once more at 0900 on May 2nd two miles off Huelo, Maui, having made some 30 miles of easting against the wind and current in the 16 or so hours since clearing the land. The next north-going tack was a much longer one of 100 miles, so that it was not until 0630 on May 3rd that we came about onto the port tack again. The wind, Piailug said, though variable in exact direction, was generally easterly, and so was not at all favorable. It was stronger, being mostly force 4 (13-16 knots). Nevertheless, *Hokule'a* continued to make some easting, her average course in Piailug's estimation being towards the 'half risen Southern Cross', *Majemehas* (about 160°). There were sunny intervals and the navigator kept track of our course to windward using both the sun and the swells as references. The canoe's speed was about 4½ knots.

At Piailug's request I visited him each day about noon to note down his observations on our progress, position and future tactics. Noon was chosen to make easier subsequent comparison with the chase boat *Meotai*'s noon positions. At midday on May 3rd, the navigator said that about 30 miles had been covered since we had tacked at dawn. He pointed towards where the invisible Big Island lay on a bearing of 170° - 175°, 10° - 15° off the starboard bow, and said it was about 70 miles away. Piailug then remarked upon the long ocean swell rolling down from the northeast, how it was unbroken by land interference, unlike the inshore lop we had lately experienced.

Hokule'a continued close-reaching on the port tack, taking advantage of every wind shift, as she was to do for the bulk of the voyage. The following morning, May 4th, I estimated that, if the wind remained steady, we should clear Hawaii without further tacking and should be abreast of the Big Island by 1500. The wind eased, however, so that when the 13,800 foot Mauna Kea broke through the clouds at midday, it was still 40 miles to the southwest and we did not come level with the mountain until dusk. The wind freshened again and

Hokule'a speeded up, so that the loom of Cape Kumukahi light on the easternmost tip of Hawaii was abaft the beam by midnight. We took our departure from the land shortly before dawn on May 5th, when Kumukahi light, then 20 miles astern to the northwest, was lost in the growing daylight.

Cartography by Dennis Nakaoka
From NAT'L GEOGRAPHIC


HOKULE'A SAILS FROM MAUI, HAWAII
MAY 1, 1976
TRACK CHART
20° N
10° N
0°
10° S
20° S
150° W
0 300
nautical miles
HAWAII
Pacific Ocean
DOLDRUMS
TRADE WINDS
CURRENTS
Equator
MAY 3
MAY 4
MAY 5
MAY 6
MAY 7
MAY 8
MAY 9
MAY 10
MAY 11
MAY 12
MAY 13
MAY 15
MAY 17
MAY 18
MAY 21
MAY 23
MAY 25
MAY 26
MAY 27
MAY 28
MAY 29
MAY 30
MAY 31
MATAIVA FIRST LANDFALL JUNE 1, 1976
MARQUESAS
SOCIETY ISLANDS
TAHITI
JUNE 4, 1976
TUAMOTUS

Recording Position and Progress in Terms of Two Systems

Piailug gave me the noon position on May 5th as '50 miles southeast of Cape Kumukahi.' Subsequently, he took South Cape, Hawaii, as his home reference, stating its distance in miles, its bearing in star points, and often in degrees as well. As we progressed further, he incorporated in his daily estimate the distance from the doldrums and the equator, and the distance and bearing of Tahiti and/or the Tuamotus.

Piailug's estimates of position were extremely accurate, as can be seen from the track chart, and were based on unremitting informed observation. Thus he was aware, by virtue of his trained reading of the messages of the stars, sun, winds and waves, of our exact course at any time of the day or night; he would often remark that a wind shift, unnoticed by me, had altered our heading.

Piailug continually updated and revised his estimates in the light of any new data that came to hand, an approach common to other star path navigators (Lewis, 1975: 47 *et seq*). A sight of the Pole Star, for instance, after several cloudy nights, would enable him to adjust his dead reckoning latitude estimations. He was not a bit concerned with where we *had* been (as perforce I was, through having to record our noon positions), only with where we were *now*, and how this should govern our future tactics.

This constant updating process seems to be characteristic of pre-instrumental navigational systems, but it is foreign to sextant and time signal instrumented mathematical navigation. In the latter, habitual recourse to such, to us familiar, stylised abstract representations of the ocean as Mercator charts, crossed by parallels and meridians and to written symbols, dictates the laying down of static sequences of position marks. Pre-instrumental preliterate navigation, on the other hand, is dynamic; fresh data are being processed all the time, not just when taking and working out a sight. As the course unrolls, the accent is kept firmly upon the here and now—the ever-changing present position.

An even more significant difference between the ancient and modern systems is that the input data of the former is made up of observed phenomena of different orders—stars, seabirds, wind, clouds, swell patterns, deep phosphorescence and so on. These are generally not subject to precise measurement, nor do they allow comparison. Yet mensuration of comparable categories is the basis of Western sciences.

Piailug's only navigational difficulties (and mine too, as a recorder) arose when he had to seek equivalents between his system and the Western one. But such problems were invariably concerned with the *expression* and the *recording* of Piailug's navigation, never with the navigation itself. The main areas of difficulty were: speed and distance in knots and nautical miles versus days' sailing; star points in degrees; noon positions in latitude and longitude as well as bearings and distance.

Speed and Distance. Piailug judged the canoe's speed by eye, with the precision that appears typical of trained Micronesian and Polynesian navigators (Lewis, 1975: 118-122). But whether he expressed the speed to himself in units other than the resultant of mean speed for a certain time, such as a day's sail or so many miles, I do not know. The only certainty is that his estimates were accurate, for they tallied with the instrument-measured distance sailed by *Meotai*. My own method of assessing this important component of dead reckoning was by counting seconds as we passed a patch of foam—6 seconds meant 5 knots and 5 seconds, 6 knots, for instance. Piailug just looked at the passing seascape, but his results were at least as accurate as mine.

In the early days at sea Piailug was not yet very sure of himself and preferred me to give my estimate of the day's run first; he would then concur. (This aroused the suspicions of certain of the inexperienced crew that the navigator was in some way being 'got at'). Piailug several times required me to confirm that the length of the voyage would be 5 times the Satawal-Saipan one and to reaffirm other background data. Later, however, he learned to express himself with confidence in miles, for the day's run, total progress and distance still to go. My estimates, which he had needed at first only to help him master an unfamiliar *mode of expression*, were no longer needed.

Star Points and Degrees. Finding the correct equivalent of a star compass point in degrees is complicated by two factors, as explained above:[15] several points are denoted by sizable constellations; some points are marked by stars that rise and sink at an acute angle. Any native navigator, of course, knows exactly which part of a constellation is the star point. Similarly, when a star rises at an acute angle to the horizon, he knows precisely when it is over the star point (Lewis, 1975: 56-7). In translating these star points into degrees, almanacs are but general guides, since they tabulate stellar bearings on the horizon. An exact comparison could, naturally, be made with a prismatic compass, but we carried no compass.

There is no contradiction in the circumstance that Piailug, who knew his star points so well that he habitually made good courses of more than 100 miles with less than 5° error (cf. Gladwin, 1970: 156, 202), should erroneously think that the bearing of the rising Antares was 90°, when in reality it was, as we have seen, 81½° (Lewis, 1975:64). Again, this in no way affected the precision with which he laid down courses, followed them and noted and remembered the smallest deviations; it only affected his expression of bearings in foreign terms.

Latitude and longitude were alien conventions to Piailug's system, except for latitude north of the equator, which he measured in *ey-ass* and expressed both in *ey-ass* and by comparison with islands where the altitude of Polaris was the same as at his position (e.g. Saipan, Satawal, Ulul). Thus, as we sailed southwards, he would observe the Big Dipper, the Southern Cross and the North Star, when it was not obscured by clouds, and announce: "Those stars are the same height as they are at Saipan. We are now opposite Saipan (or Satawal)."

Apart from expressing north latitude in *ey-ass* and by comparison with known islands, Piailug described *Hokule'a's* position in terms of star compass bearings (and less happily in degrees) to and from the home, goal and *etak* islands, and in miles or days' sailing from Hawaii and to Tahiti—not in latitude and longitude. I logged our position in similar terms. For example, on May 15th I recorded: '950 miles S.E. by S. of South Cape, Hawaii, Tahiti bears 190°, distant 1560 miles.' But, for the record, I also set down our noon positions as latitudes and longitudes. Familiar as I was with the Western system, this 'translation' introduced errors into my reckoning on two counts. First, I updated our positions in the light of such definitive data as the height of a tardily reappearing North Star or an observation of one of the few available zenith stars. I would already have committed to paper the past day's noon positions and runs and, while it was easy enough to amend the latitudes, it was time consuming to reconcile these with the carefully reckoned daily runs. I made little effort, therefore, to attain absolute consistency between amended noon positions and estimated daily runs because, like Piailug, I felt our current position to be the all important one. Second, I had not troubled very much to memorize longitudes before the trip. South Cape, Hawaii, Piailug's home reference, I thought was in 155° west longitude when it was actually in 155° .40'—37 miles further west. This accounts for the glaring error in my 5th May noon longitude of

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[15] See 'Expanding Piailug's geographical terms of reference'.

153° .00′ W., which places us too far offshore (see track chart), when we *knew* from Kumukahi light where we were to within a few miles—in bearings from the land. It was simply my translation of this known position into longitude from memory that went wrong. In sum, Piailug navigated with confidence and success entirely by the application and 'expansion' of his traditional system. The difficulty arose in recording positions rather than in estimating them, and this difficulty stemmed from the problems of comparing two dissimilar and partly incompatible systems.[16]

Down Through the Northeast Trades

Hokule'a's noon position on May 6th was logged as: '120 miles east-southeast of South Cape, Hawaii', and also as '17° N., 152° .20′ W.' The estimate of latitude was later found, by a good view of the Pole Star, to be a shade too optimistic. Nevertheless, 17° .15′, after the 20° odd at Maui, represented real progress and we were heartened. Piailug averred that the shape of the waves showed that a strong current was flowing, though the waves' contours did not reveal its direction.

Three wind changes between northeast and east-southeast occurred in the course of 24 hours; we managed to make a degree and a half of southing (90 miles), though less than the desired easting.

The most memorable feature of May 7th was Piailug's analysis of the swells. Now that the canoe was well clear of land he spent more time than ever gazing intently at the sea, sorting out the swell pattern and analysing its components. Five swells were running, he said. Not all were present all the time and some could be detected only with patience and by the most careful observation. The swells were: a long, low northerly swell; a prominent and choppy northeast swell; a similar, but rather smaller swell from *Mailob* (Satawalese—approximately east-by-north); a much weaker southeast swell; a long, low southerly swell, only periodically apparent and even less noticeable than the northerly one.[17] These swells helped Piailug maintain course by night and by day. They supplemented the stars. The slow-arching sun was the primary guide, whose precise bearing (in terms of star compass points) at any given moment, Piailug could judge to a nicety.

May 8th marked the end of the first week at sea. Piailug and I were of one mind: though we had covered about 745 miles in all, more than half of this mileage had been tacking round the Hawaiian Islands, so we were only 330 miles southeast-by-south, 145°, from South Cape, Hawaii or towards 10° south of *Tumur* (Antares, 135°).

The following day's run added 120 miles to this total. At the note taking Piailug discussed measurements of distance.

"In Satawal we use *etak* to measure distance. Some 'navigators' talk in miles now; one day's sailing with a good wind is about 100 miles in a Satawal canoe, this maybe five *etak*.[18] Our own *etak*, the Marquesas, it move very little so far." Piailug illustrated his point by taking a few short shuffling steps. "Wind here like Satawal to Saipan, but here more steady. Current more strong here, I think."

We went on to talk of the doldrums, which we expected to enter before the week was out. We calculated the speed we were making. The northeast wind had freshened to force 5 (17-21 knots) and *Hokule'a* was butting her way through the seas at a good 7 knots.

It might be instructive at this point to compare *Hokule'a's* performance with *Meotai's*, the chase boat, since both craft were the same overall length. the Ketch *Meotai* decisively outpointed the double canoe and was a good deal faster to windward. *Meotai* had to keep under very easy sail to avoid overrunning her charge. Downwind, the two vessels were more evenly matched and *Hokule'a* may even have been the faster, but occasions for comparison were too few to allow us to be definite.

Time estimation in the absence of clocks was a practical problem on *Hokule'a* only in so far as change of watches was concerned. In timing the changes, we were aided by such observations as that, at the beginning of the voyage, the sun rose at 0600 and set at 1900, and that the sun and equatorial stars progressed across the sky about 15° each hour. One helpful fact that Piailug noted was that the star Arcturus (*Hokule'a* in Hawaiian) set an hour before dawn, or approximately 0500, at the start; two hours before dawn, or 0400, two weeks later. Overcast nights became the norm on the second week out and a certain acrimony often developed between the two watches as to the times they should be relieved. The situation was not without humor. Once Piailug was awakened to adjudicate. He barely glanced at the impenetrable and uninformative overcast before announcing,

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[16] One instrument-conditioned commentator, who had neither read the relevant literature nor met Pacific Island navigators, asserted that my pre-voyage examination of a Mercator chart (in default of long vanished Polynesian sea lore) was a 'valuable navigational aid' when, it will be obvious from the above, the enforced use of data from an alien system was our major navigational headache.

[17] The low northerly swell had come a long way, presumably from the Northern Hemisphere westerlies; both the N.E. and the E. by N. swells were products of the N.E. Trades; the weaker S.E. swell originated in the S.E. Trades; most distant of all in origin, the southerly swell can only have arisen from storms in the remote Southern Ocean.

[18] See above. An *etak* is a variable distance, but about 20 miles is a preferred average (Lewis, 1975: 135).

"Five minutes to four!"

Piailug was joking, but the claim has been seriously advanced that old-time Pacific navigators possessed an innate time judging ability of a high order, and that this would enable them to estimate their longitude (Gatty, 1958: 41). While there is no doubt as to the existence of a human biological clock or clocks to control rhythmic functions, we possess nothing, to my knowledge, even remotely comparable to the specific navigational mechanisms of migratory creatures. Any idea that the human time sense is of such a character that would allow longitude to be even roughly determined seems quite untenable. Despite impractical schemes foisted upon long dead Polynesian navigators by ingenious Westerners (e.g. Reche, 1927), it must be stressed that longitude can only be measured by instruments.

Persistent cloud, particularly low down in the north, was a feature of the second week at sea. Thus, while the higher constellations broke through at intervals, the Pole Star was not glimpsed once for eight consecutive nights. On the evening of May 11th, the fifth without Polaris, Piailug assessed our latitude by the comparative heights of the Big Dipper and Southern Cross. He expressed himself in these terms: "No see *Fiis e Magid* (Polaris), Big Dipper higher than at Satawal but much lower than at Hawaii and lower than at Saipan. I think nearly the same as at Satawal, maybe not so much as half an *ey-ass* higher. I see Southern Cross high like at Satawal." There is an apparent discrepancy between Piailug's two assertions. Satawal lies in 7½° N. The altitude of the Southern Cross suggested that we were about this latitude; the Big Dipper that we were 'not so much as half an *ey-ass*,' say 4° further north, or in 11½° N. But if we average out the two positions we come up with 9½° N. and, in fact, in the late evening when this conversation took place *Hokule'a's* latitude was 9° N.

Is this mere juggling with figures? No, the trained Polynesian or Micronesian navigator orients himself, in both direction and latitude, by the '*feel of the whole sky*'. This was not the sky picture as seen from Saipan in 15° N., nor was it yet quite the Satawal sky. Polaris being invisible, no precise estimation was possible, but Piailug had studied the *general appearance* of the heavens and produced a not too inaccurate answer. (It is perhaps worth mentioning that my own dead reckoning, in the absence of Pole Star or zenith star confirmation, put us a full 2°—120 miles—too far north!)

When Polaris did finally appear on 15th May, it became clear to me also that I had been underestimating the canoe's speed and that our recorded noon positions had been logged 2° too far north and required revision. Our course had been southeast, so that for every 120 miles sailed our southing was 60 miles or 1° latitude. Our runs must have averaged nearer 140 miles than 120.

May 15th was the end of the second week at sea and it was our second day in the doldrums. I placed us in 6° N. (we were actually 5° .22', 40 miles further south). According to Piailug, we were about 1,000 miles southeast by south of South Cape, Hawaii. Tahiti bore 190° (Piailug used degrees this time), distant some 1,500 miles.

Day after day we had been visited by mid-ocean birds like petrels and sheerwaters. But boobies also followed us; on the 14th we counted no fewer than 13 at one time. Boobies are usually found no more than 50 miles from land, yet these were a good 700 miles from the nearest island, the Line Islands to the west. This was unprecedented in Piailug's experience and my own. I can offer no explanation.

The Doldrums and through the Southeast Trades

Hokule'a was a week in the doldrums, creeping along from 5° .30' N., to 2° .44' N. plagued by calms and fickle breezes, making good an average of 24 miles a day southward, and helped by the Equatorial Countercurrent, gaining 120 miles of easting.

This was the time when the myth of trans-oceanic paddling was laid—one hopes for good. The myth is current mainly in Hawaii, for the reason that Hawaiian canoes have been paddling canoes, with or without auxiliary sail, for the last five centuries. These canoes are superbly adapted for surf landings but have little in common with Polynesian deep-sea voyaging vessels (including, presumably, those of ancient Tahiti-Hawaii). In the doldrums it became so obvious that a high-sided sailing vessel like *Hokule'a* did not lend itself to paddling, that not even the most ardent beach boys in the crew bothered to make the attempt.

The doldrum interlude was pleasant enough, especially as the navigators could relax in the comfortable assumption of a favorable current.

Nevertheless all welcomed the Southeast Trades that set in on May 21st.

Noon on the 22nd marked the end of the 3rd week. The bearings of Tahiti and of Hawaii were much the same as they had been the week before, Piailug said, but we were now well past the half way mark and very near the equator. I put us in 0° .40′ N. 145° .50′ W. *Meotai*'s figure turned out to be 0° .46′ N., 144° .39′ W. The Pole Star was clearly visible from very low above the horizon. The precision contrasts with the error of 70 miles in longitude which, of course, was be dead reckoning alone, no non-instrumental confirmation being possible.

From then on our preferred course would be toward *Majemehas*, the half-risen Southern Cross (approximately south-southeast), as Piailug had laid down back in Hawaii. Heading as we were nearly southward, instead of southeast as we had been, *Hokule'a* crossed the latitude lines nearly twice as quickly—120 miles sailed meant almost 2° (120 miles) of southing now, rather than 60 miles as on the former southeast course.

With some dismay, however, we found that the wind was unseasonably southerly, so that we were headed and driven off course to the westward. This unfortunate displacement was augmented by the west-flowing South Equatorial Current, the trade wind's concomitant. To exactly what extent we were being displaced we could not know for, while the course steered was easy enough to compute, the strength of the current was a matter for intelligent guesswork. But on some days we knew very well that we must be losing 60 miles and more of precious easting. To tack would have been pointless in face of the strong current. We had no alternative but to continue on the port tack and hope that the wind would eventually free to east-southeast. Should the wind remain unfavorable Piailug had his strategy ready (see below, 'landfall').

It is necessary at this point to record two unfortunate occurrences. One stemmed from well meaning efforts to keep up the morale of the somewhat land-dependent crew. *Meotai* tape recorded personal messages and also (rather nauseating) broadcasts extolling the 'instant heroes' of *Hokule'a*. The tapes were to be carefully screened to exclude navigational information before being ferried across to the canoe. Unfortunately a sentence was let slip to the effect that *Hokule'a* was 900 miles southeast of Hawaii. Annoying as it was, this statement of our position was a good deal more vague than our own reckoning and told us precisely nothing.

The second incident could have been disastrous. Piailug and I only learned after our landfall that a small transistor radio had been smuggled aboard the canoe at Maui, and that certain of the crew had been surreptitiously listening to some of the Hawaiian radio reports of *Hokule'a*'s progress. Fortunately, in their navigational ignorance, they did not try to beam their directional antenna onto Tahiti to find its bearing. Nor did any word of their activities reach Piailug, Rodo Williams or me prior to our landfall.

Our crossing of the equator was logged as 0100 May 23rd, though *Meotai*'s figures show that we actually crossed the line at 0400; we were some 15 miles north of where we thought we were. True to form, our estimated longitude was much more in error, for we were 79 miles more favorably placed—up wind and up current—than I supposed. Our bearing from Hawaii, said Piailug, was a shade more than 150°, the distance 1,250 miles. Tahiti was 1,000 miles south and a little west of us.

Sometimes the wind freed, allowing us to resume Piailug's chosen course to the south-southeast, but more often we were headed off towards the west. Comparison of track charts shows that Piailug's judgment of direction was exact; it was my judgment of current strength, and hence longitude, that was faulty. I consistently put us too far west.

The log at this time became filled with references to latitude determination by overhead or zenith stars. Our main evidence for the use of zenith stars in navigation comes from Tonga and Tikopia (Lewis, 1975:235-8). Pointers as to their use on the long-abandoned Tahiti-Hawaii voyages are, not unnaturally in view of the lapse of five centuries, indirect.

The mid-nineteenth Hawaiian scholar Kepelino referred to 'stars that are suspended (kau) severally over the various lands, such as Hoku-lea in the Hawaiian islands, and the Southern Cross over the lands of Tahiti', etc. (1932: 13, 82, 83. Translated for the author by S. Elbert). The star Hokule'a is Arcturus which, in 1000 A.D., had a declination of 24° .39′ N., and so passed overhead about 100 miles north of the archipelago.[19]

An indication that zenith stars had a meaning for the navigator-astronomers of the Tahitian group is a chant called 'Pillars of the Sky,' recited in 1818 at Bora Bora by an old woman named Rua Nui (Henry, 1907: 101-2). The 'pillars of the sky' were stars which propped up the sky from the earth, an important function. It seems more than coincidence that the 1000 A.D. Tahiti-Hawaii and Tahiti-Rarotonga-New Zealand axes, namely: Arcturus close north of Hawaii; Aldebaran 300 miles south of Hawaii; Spica, Alphard, Procyon and Betelgeuse over various of the Line Islands; Antares over Tubuai Archipelago and Rarotonga; Phact over northern New Zealand. The remaining two pillars, Polaris and Dubhe, indicate north.

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[19] The significance of Kepelino's mention of the Southern Cross, a steering star, in the same sentence as a zenith star is discussed by Lewis (1975:239).

The Line Islands, uninhabited at European contact but bearing evidence of former Polynesian occupation (Emory, 1934: 1-4, 37-40), lie to leeward of the Hawaii-Tahiti route between about 7° north and 6° south. Polaris, of course, is not visible from Tahiti and is not raised until half way to Hawaii. Tahitian knowledge of the very existence of this star, therefore, let alone the place accorded to it in Tahitian cosmology, is a significant pointer to former Tahitian voyaging into the distant north.

Zenith stars have changed position since the last double canoe preceded *Hokule'a* along the Hawaii-Tahiti track. Some stars that would have been potentially useful to us, like Sirius which passes over Tahiti, culminated in daylight during our voyage and were hidden. The zenith stars we actually used aboard *Hokule'a* were: Zubenelshemali, or β Libra, declination 9° .20′ S. Spica, declination 11°S. Zubenelgenubi, or γ Libra, declination 16°S. Gienah, declination 17° .25′ S.

The first significant zenith observation was on the night of May 29th. *Hokule'a* had been four weeks at sea. For the two preceding days some freeing of the wind had allowed her to make good a southerly course. We were still a little west of Tahiti, Piailug thought, and Hawaii was more than 2000 miles astern. I was all too well aware that a small uninhabited island called Caroline (not to be confused with the distant archipelago where Piailug lived), lay in latitude 10°S somewhere in our lee. My reckoning placed it at 30 miles to the west of us (actually it was 80), and I had advised a special lookout that night. We were steering nearly south. I peered up along the almost vertical mast while Spica (declination 11° S.) slowly mounted towards its zenith. When Spica ultimately passed overhead not more than 1/2° south of the masthead, I knew we must be in something like 10° .30′ south latitude—in other words, we were safely past the atoll, which was in 10° S., and could relax.

I was amused at the thought that *Meotai's* navigator, with only his noon sextant sight of eleven hours earlier to go on had probably less idea of his precise latitude than we had. He was aware, though, as we were not, that the atoll was well off to the west. This zenith observation enabled Piailug to confirm his dead reckoning that our distance from Tahiti at that time was 450 miles.

Landfall

Piailug then outlined his strategy for landfall, a scheme of masterly simplicity, which would guarantee our finding land within a week or at most ten days, no matter what our current displacement had been.

Should the wind head us again we might be forced west of the Tahiti group—the Society Islands. Alternatively, it was conceivable if less likely, that we might sail unaware through the gap between the Tuamotus and Tahiti. In either case, our procedure would be the same. It was Sunday (30th May). If we failed to sight any islands we would continue on our southerly course until Thursday, by which time we would be well south of the Tahitian and Tuamotuan archipelagos. We should then come about onto the starboard tack and head north-eastward. The two archipelagos lay in an east-west chain many hundreds of miles long. Cutting through them and their screens of birds and swell phenomena obliquely in this manner we could not fail to find land.[20]

This procedure, which certainly would have worked, did not prove necessary. Zenith star observation on the night of May 31st was difficult on account of the wind having freed, allowing us to sail southeast. Previous experience had shown that zenith star observations are exceedingly hard to make unless the canoe is steered towards a cardinal point (Lewis, 1975:244-6). The best estimate I could make was that Zubenelgenubi (dec 16° S.) seemed to pass a shade less than 1° south of us, and to log our latitude as '15° S. plus (1/2° too much).

Shortly before dusk that evening the familiar swell from the southeast had been abruptly cut off. We could only have sailed into the shel[illegible] the Tuamotus. Then Rodo Williams, the Tahitian navigator, spotted two *ititae* a species of tern that, he explained, did not fly further than 30 miles from land.

That night the watch on deck, cautioned to keep a good lookout, stared intently into the darkness.

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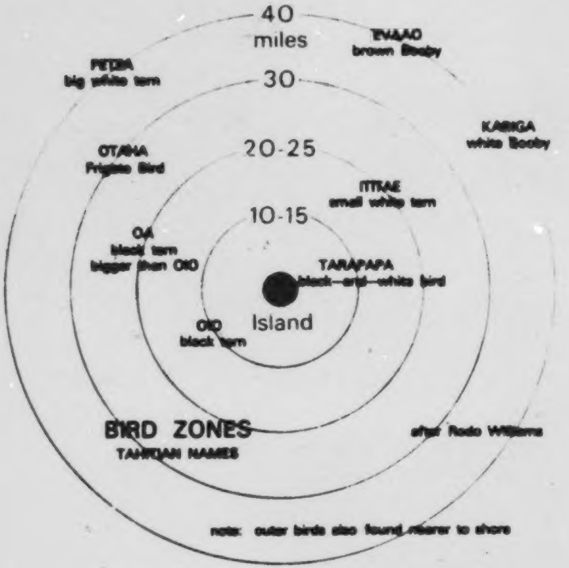
[20] An identical oblique approach, this time to a north-south island chain, was made by the navigator Hipour when he made the first Carolines-Saipan voyage for three generations in 1969 (Lewis, 1975:217-8).

About 0300 on June 1st a black line became visible on the port bow. It steadily grew darker and more solid—an atoll. We came about and hove-to to await daylight. The three navigators discussed the island's identity. We agreed that our latitude of something under 16° S. meant that we were south of the northernmost Tuamotuan atolls but still well north of Tahiti. The only atolls that would fit the bill seemed to be Tupai, north of Bora Bora, and Mataiva, westernmost of the Tuamotus.



Cartography by Dennis Nakaoka

It was here that Rodo Williams' local knowledge and expert dead reckoning came into its own. I was inclined to favor Tupai. According to Rodo's dead reckoning, on the contrary, we had regained a good deal of the easting we had lost and should be near the longitude of the Tuamotus. Moreover, the *itatae* and the swell shadow both pointed to the proximity of the Tuamotus. He plumped firmly for Mataiva. Daylight confirmed his judgement. Here was a solitary atoll with no tall Bora Bora rising behind it—it was indeed Mataiva.

With our arrival in Mataiva the navigational portion of the *Hokule'a* a voyage was virtually at an end. There remained but a straightforward 170 miles to the lofty island of Tahiti, and this was accomplished after an overnight stop. The warmth of our welcome on the little atoll and at Papeete alike, was something none of us will ever forget.

WHAT PIAILUG HAS TAUGHT US

Historical Connotations of Ancient Voyaging.

The relative ease with which Piailug expanded his geographical range, his total command of the situation throughout the voyage (including the procedure to be adopted should Tahiti be bypassed), and the proven applicability of his system to the wider ocean are all strongly suggestive of the one-time existence of a Pacific-wide navigational system. While Carolinian navigation would not appear to be so readily and universally applicable to other seas as is latitude and longitude, nevertheless, Piailug did conclusively demonstrate how easily his system could be extended throughout the very considerable realm of Polynesian and Micronesian dispersal. Other evidence that we have discussed points towards the essential elements of this system having had an Indo-Pacific provenance and possibly having been diffused in related forms throughout the off-shore seaways of the ancient world.

To avoid confusion, it must be stressed that the concepts and methods of non-instrumental navigation, transmitted among pre-literate societies by precept and practice, could achieve nothing like the uniformity of their instrumental counterparts with their standardised equipment and equally universal books and tables. The former would inevitably exhibit great local and regional variation due to diverse geographical, cultural and historical circumstances (Lewis, 1975; Lewis, 1976).

Piailug's own assessment of the value of the *Hokule'a* voyage was rather more negative than mine (Ronck, 1976; Piailug, 1976, transcripts of two post-voyage tape recordings). The outside world is, however, a good deal less familiar with the nature and efficacy of pre-instrumental navigation than is Piailug. And after all, it was to acquaint this wider public with some idea of the venerable skills of which Piailug is a distinguished bearer — skills once shared by the sea-going ancestors of a good part of mankind — that this account was written.

Wider Connotations for Cross-Cultural Learning.

Piailug's achievement brings home certain lessons that are more generally applicable and of more practical moment than the ancient art of navigation *per se*. It has become abundantly clear, for instance, that literacy is not a necessary adjunct to a learned man; that an orally transmitted system of thought and technology may be quite as valid as a written one; that flexibility is not necessarily absent in a traditional system—though there are very real problems of outside interaction.

These issues I must leave to the consideration of others more intimately concerned with two-way cultural interaction. The issues are of some moment to all who have to do with bi-cultural education, whether for Puerto Ricans in the U.S.A. or Aborigines in Australia. More especially are they relevant to the work of my colleagues in the Culture Learning Institute of the East-West Center, who are concerned with the interplay of culture one with the other, rejecting as totally undesirable the pernicious imposition of one culture upon another. The Center's far-sighted policy of appointing the marginally literate, but learned and accomplished, Piailug to a Research Fellowship has indeed brought rich dividends.

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THE NEW WORLD ORDER AND THE GLOBALIZATION OF SOCIAL SCIENCE: SOME IMPLICATIONS FOR TEACHING CROSS-CULTURALLY[1]

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Historically, ideas originating in one cultural setting have been exchanged and shared with people in other cultures, often being modified in the process. The international exchange of academic knowledge and theories is today encouraged by the proliferation of academic institutions and the mobility of academics and well-read elites. The implications of this international exchange in the social science disciplines needs to be more fully explored and understood, especially from the perspective of questions and insights gained from the sociology of sociology and challenges posed by the contrasting wealth and poverty seen in the world today. In the 1970's some social scientists have focused their attention on social science transactions between industrialized and less industrialized countries (Alatas, 1972, 1975; Alger and Lyons, 1974; Ashraf, 1975; Bengolia and Lyons, 1974; Bodenheimer, 1970; Forrester, 1974; Goonatilake, 1975; Innayatullah, 1975). The complexity and subtlety of such transactions has been explored by some (Guthrie, 1976; Kumar, 1975).

The social sciences as we know them today originated in the industrialized nations of Europe and were transported to Europe's colonies in North America, Asia, Africa and Latin America. Colonialism, especially through its legitimization of an essentially Western educational system, and nationalism and the move towards independence led to the contemporary institutionalization of social science in the Third World. This latter development in turn helped legitimize the expansion of social science institutions and influence in North America and Europe. The historical ties between colonialism and imperialism and the internationalization of social science theories and institutions are clear.

What is not so clear is the effect present trends will have on these disciplines, if and how they will emerge from their European roots, become sufficiently self-concious and self-critical to be genuinely international and contribute to the development of an interdependent and more equal world order. In this paper I attempt to summarize the dialogue on this question and its implications for the cross-cultural teaching of social sciences, sociology in particular.

I have organized the paper into four sections. In section I some of the basic points underlying the debate on the need for reorganizing the present economic order are discussed. One of the major conclusions derived from this debate is that since influential theories and concepts of development that have been formulated in the United States and Western countries during the period between 1950 and 1970 are not suited to the situation in many less industrialized countries, it is necessary, if liberation and self-reliance are to be achieved to systematically evaluate the intellectual biases of these theories. This realization on the part of leaders and social scientists in countries in Asia, Africa, and Latin America was further enhanced by the development of dependency theories, mainly in Latin America, and the Critical Theory at Frankfurt. Therefore, in section II some of the salient points of these theories are discussed and contrasted with the major points of the theories developed in the United States. In section III some of the main points emerging from the discussions in the preceding sections are summarized and it is concluded that social scientists employ theoretical orientations that are culturally bounded and relative. Following this conclusion is a question: How can we use culturally bounded social science knowledge to educate the younger generation in a manner that retains its critical function and does not become an instrument of indoctrination and propaganda in the hands of the powerful? In section IV it is suggested that a "comparative sociology of sociology" approach to teaching can overcome many of the intellectual biases inherent in theories and concepts developed in different national and cultural settings, and that by using methods and techniques of comparison and synthesis it is possible to generate culturally and nationally sensitized "global" or "transnational" sociological knowledge.

I

A socially and politically conscious debate on the need for reorganizing the present economic world order first took place at the United Nations Conference on Trade and Development in 1964.[2] Since then, a voluminous literature has appeared in which various issues pertaining to the building of a new world order have been discussed at length. This paper makes no attempt to cover all aspects of the ongoing debate. However, some points are discussed as background for arguments made later.

It is now widely recognized that in the present world economic order some countries are rich while most others are poor. Moreover, the gap between them is increasing. One of the consequences of such a situation is that in the late 1960's and early 1970's the views held by the rich and the poor nations on such topics as development and international relations have become increasingly polarized. Further, the "oil crisis" of 1973-74 intensified the confrontation between the beneficiaries and the victims of the existing international order.

The present world economic system was put together in 1944 and 1945 and at that time the views and the interests of the poor nations were simply ignored. The characteristic feature of the present economic order is that 70 percent of the world's population awards 70 percent of the world's income to the other 30 percent of its inhabitants. Under this system, countries and areas which are on the periphery serve as sources of raw material, export markets, and places for high return investments for countries at the center. Thus, the partners involved in economic activities have an unequal status which results in the exploitation of the weaker partners. The overall issue in the late seventies, then, is the validity, or at least the acceptability to the majority of the world's inhabitants, of such a lopsided world order. Are the rich willing to respond to the demands of the poor nations, the nations at the periphery? Can the interests and views of the poor be simply ignored?

One of the main thrusts of the debate is the realization, through the experience of two decades of development initiated by the United Nations in 1950 and 1960, that development as conceived in the past has not achieved its goals. This has given rise to two new concepts: (1) "global interdependence," and (2) "self-reliant development." The first one is receiving increasing attention in the North (i.e., the developed countries), while the second is becoming a major concern in the South (i.e., the developing nations). North-South relations are being discussed in the midst of the new awareness which aims for genuine interdependence and international economic justice.

The experience of the developing countries with the development process over the last two decades has made them aware of two things: (1) the concepts, policies, and patterns of development borrowed from developed nations are not often suited to their needs; and (2) patterns of international economic cooperation that continue to make aid, trade and transfer of technology a major part of the development process are instruments of domination used by the industrialized nations. Those who advocate self-reliant development suggest that: (1) developing nations should re-evaluate their ambition to become high production/high consumption societies like Japan, West Germany or the United States; (2) the idea of growth first and equal distribution later be questioned because experience has frequently shown that increases in GNP increase and institutionalize economic and social inequalities; and (3) that without a fundamental transformation of the social institutional framework the attainment of social justice, self-reliance and a decent standard of living for the masses may not be possible. The old insistence on a rising GNP as a panacea for all ills is thoroughly examined and found wanting.

Those who insist on building a genuinely interdependent world economic order[3] point out the very unequal international effects of dependence in the past and present. They feel that the promise of gains to all parties through industrialization has inevitably led to the exploitation of the weaker partner nations. They would also agree with the Hammarskjold Foundation which concluded that, "integration-increased dependence and reduced the capacity for self-reliance" (cited by Barraclough, 1976, p. 36). They feel that basic changes in the world economic order are necessary and that the orientation and biases of much of the literature on development must be challenged (Erb and Kallab, 1975, p. 181).

II

In this section some of the salient points of three theories of society and history—the "conventional" theory, the dependency theory, and the Critical Theory—are discussed with two purposes in mind. The first is to illustrate the way social scientists in many countries proceeded to achieve the task of systematic evaluation of intellectual biases in the development theories propounded in the West. The second is to reflect on the implications of some of the insights gained in the process of comparing and contrasting these theories for teaching social sciences cross-culturally.

It should be pointed out at the outset that the three theories ("Conventional," Dependency and Critical) provide contrasting diagnoses of underdevelopment and development of societies on a world scale. This adds to the already heterogeneous nature of social science knowledge (discussed below) and poses two serious questions. First, what should be consciously selected and communicated, for the purpose of socialization, to both the general public and to those who want to become professional social scientists and educators? Second, what are the implications that contrastive analyses of underdevelopment and development have for the re-education of educators themselves?

Conventional theories rely on two models of change. The first model is based on the older economic theories which saw change as a continuum defined by growth and evolution. The second model, actually a sub-type of the first, is based on the more recent sociological and political theories, which saw change in a sequence of stages. Irrespective of disciplines, the main point is that these models tended to divide world societies into two opposite poles—traditional and modern—and saw social development as a transitional process in which societies in the Third World are passing through the same stages that today's developed societies of the West went through. The essence of this kind of perspective is that in due time the traditional societies would become somewhat like today's Western societies (i.e. would be a variant of a Western type) characterized by high mass consumption within a capitalist mode. Moreover, this perspective tended to see problems or "obstacles" to underdevelopment wholly within the confines of national boundaries of the traditional societies. The cause of underdevelopment (or development) was to be found in such factors as slowness of economic production, attitudes, traditional value systems and cultures, rigid personality traits, pre-capitalistic feudal systems of social stratification, underdeveloped political systems, religion, archaic institutions and the like. Development was seen as an evolutionary process passing through certain necessary stages uniformly in all societies. The process could be accelerated if only the right approach, based upon the experience of the already developed capitalist societies, was adopted. The policy recommendation based upon this perspective was that in order to develop, the societies in the Third World should eliminate variables of underdevelopment (particularism, ascription, and functional diffuseness) which are in direct opposition to the pattern variables exhibited by the developed capitalist countries (universalism, achievement orientation, and functional specificity). Frank (1969) has critically evaluated several of the societal models of development that were the product of this perspective. Some well known models are: Apter (1965), Black (1966), Bendix (1964), Blumer (1966), Eisenstadt (1966), Hagen (1962) Hoselitz (1960), Lerner (1958), Levy, Jr. (1966), McClelland (1961), Moore and Feldman (1960), Nash (1966), Parsons (1960, 1951), and Rostow (1960).

The movement toward creating a new world order led by the political leaders and the intellectuals of the Third World countries grew out of the realization that the policies based on the development models drawn from the conventional theories could not yield results. The reason for this, it is argued, is that these models were based upon theories developed in Western societies which reflected the unique historical experience of these societies. These theories were not developed to meet the societal aspirations and needs of non-Western societies. On the contrary, they conceptualized non-Western societies within the framework of Western aspirations and needs. By doing this, it is asserted, the Western theorists and theories of modernization created distorted images of non-Western societies, which became, through mass socialization, part of the uncritical consciousness of the masses both in the Western and non-Western societies. Posing of the problem of underdevelopment in the form of self-reliant development and genuine interdependency, it is believed, exposes the nature of the processes that have produced distorted images in the past and continue to produce them even today. The notion of self-reliant development and similar concepts, when contrasted with concepts drawn from the conventional theories, contributes to the formation of critical consciousness. This consciousness directs the energy and efforts of Third World intellectuals, leaders and many segments of the general public towards reducing the gap between their societies and the industrialized societies. No doubt, dependency theory has contributed to the increased critical consciousness of the leaders and masses alike in both the industrialized and less industrialized countries.

The Dependency theory[4] was developed in the sixties by a group of Latin American scholars as a reaction to conventional theories of society and history and to the development policies drawn from these theories. It was seen by many scholars as a theory providing necessary correctives to the conventional theories of development and underdevelopment.

According to dependency theorists, the difficulties with conventional theories were of an empirical and historical nature. In Sunkel's (see Smith, Jr. 1972, p. 237) words: "—the formative stages of development and the present structure of the underdeveloped countries are radically different from the assumptions implicit in such theoretical logic." And in Frank's (1969, p. 21) words:

> "On critical examination, this . . . sociology of development (i.e., conventional theories of development) is found to be empirically invalid when confronted with reality, theoretically inadequate in terms of its own classical social scientific standards, and policy-wise ineffective for pursuing its supposed intentions of promoting the development of the underdeveloped countries. Furthermore, the inadequacy grows along with the development of the society which produces it. Like the underdeveloped society to which it is applied, this sociology is becoming increasingly underdeveloped."

An often quoted definition of dependency is given by Santos:

> "By dependency we mean a situation in which the economy of certain countries is conditioned by the development and expansion of another economy to which the former is subjected. The relations of interdependence between two or more economies, and between these and world trade, assumes the form of dependence when some countries (the dominant ones) can expand and be self-sustaining, while other countries (the dependent ones) can do this only as a reflection of that expansion, which can have either a positive or a negative effect on their immediate development." (see Wilber, 1973, p. 109).

Other theorists who are not associated with the Latin American group have isolated five different types of dominance-dependence relations among nations. The five types of imperialism that have been distinguished by Galtung (1971, p. 83) are: political, military, economic, cultural and communication. He defines imperialism as:

> "one way in which the Center nation has power over the Periphery nation, so as to bring about a condition of disharmony of interest between them." More explicitly, "Imperialism is a relation between a Center and a Periphery nation so that (1) there is harmony of interest between the Center nation and the center in the Periphery nation, (2) there is more harmony of interest within the Periphery nation than within the Center nation, (3) there is disharmony of interest between the periphery in the Center nation and the periphery in the Periphery nation."

While some Latin American dependency theorists work within the Marxist tradition and give primacy to economic factors, it seems that in Galtung's framework the different forms of imperialism are not specifically based on the economic factor. This is not to say that Galtung undermines the economic factor and that Latin American theorists ignore the non-economic factors. Both groups of theorists are aware of the interaction of these factors.

This exposes the fact that there are different phases in the development of the dependency theory both within and without Latin America. Dependency theory is not a single, unified theoretical structure, but consists of the writings of a group of people in Latin America and elsewhere who differ from each other on a number of crucial points. Moreover, dependency as a concept has a number of dimensions—economic, technological, education, cultural, communication, etc. Essentially, the theory is based on the analysis of advanced capitalism and state socialism. It inevitably involves a class analysis of societies and the analysis of historically evolved linkages between the Center and the Periphery economies. Development and underdevelopment are seen as two sides of the same coin. It emphasizes the inherently exploitative nature of the dominance-dependence relationship and attaches great significance to the concept of structural power in the analysis of it. The theory is seen as corrective to the conventional theories of society and development in the sense that it sees obstacles to development not only to be found within the underdeveloped society but more so in the imperialist expansion of the Central, industrialized, advanced capitalist and state socialist societies.

We do not need to discuss various arguments between the two groups of social scientists in great detail. What is of more importance, for pedagogical purposes, is to grasp the general orientation of the social scientists working in the two traditions being discussed here. This is especially so when we know that the production of sociological knowledge is closely linked to the orientations and styles adopted by the sociologists, a point that is discussed below.

Social scientists working within the framework of the dependency theory believe that scientific models used by conventional theorists are "based upon a flawed self-understanding and an interest in control, manipulation, prediction and technocratic forms of consciousness and instrumental rationality" (Smith, Jr., 1972, p. 238). Dependency theorists actively reject that version of science "that requires individual competition, isolation, detachment, and hierarchy in the practice of intellectual work" (p. 239). On the contrary, they emphasize "the need for a 'sociology of Crisis' and 'committed' sciences of development instead of imitation of supposedly ideologically disengaged modes of scholarship backed up by the prestige ordering of the international social science community dominated by the United States (p. 238). They also assert that the social science interpretations of development and underdevelopment influenced by the trends in conventional theories developed in the Central nations were partial, ahistorical, ideological and mechanistic (p. 237).

At a more fundamental level the differences between social scientists working within the dependency and the conventional modes center around the issues of the model of science, the structure and organization of institutions of higher learning and research, and the personal role and styles of work of social scientists. Dependency theorists are concerned with examining the expansion of the Central, industrialized capitalist and state socialist societies and the implications of that expansion in Peripheral nations for the production, diffusion, and use of social science knowledge. They assert that in the area of social sciences, as in other spheres, the flow of knowledge from the Central nations to the Periphery nations inhibits the development of indigenous social science knowledge, thus generating a system of intellectual dependency that legitimizes economic and political domination by the Central nations (Kumar, 1976).

These theorists, then, are concerned with the implications for the Periphery nations of the new international linkages among scholars and new lines of intellectual, scientific and cultural domination and homogenization. The major concern in this area is to see that universities and research centers in the Periphery are not dominated by the Center in a manner that will inhibit the autonomous development of these institutions and what they produce. They claim to work within the framework of "interest" which is directed towards liberating the intellectual work from domination by the Center.

Besides the dependency theorists in Latin America and North America there is another group of social scientists who want to understand the nature of domination or the socially unnecessary constraints on human freedom, especially in the advanced capitalist and socialist systems. These theorists are associated with Critical Theory or the Frankfurt School.[5] Like the dependency theorists, but in a very different way,[6] these theorists have come to the conclusion that the American-dominated world capitalist system remains the basic framework for global development. This system, according to them, sets the ground rules for socio-economic development in other nations. This does not mean that the Critical Theorists are less interested in the expansionist tendencies of state socialism as far as its socially unnecessary constraints on human potential are concerned. Basically, the Critical Theory[7] addresses itself to the problem of liberation from a higher level of industrial domination—socialist or capitalist. Within the framework of Critical Theory, the emancipation from dominance means: (1) freedom to use the infinite possibilities of an advanced civilization; (2) liberty to participate in the control of production; (3) freedom to become involved in meaningful work and to use leisure time in a non-coerced or manipulated manner; and (4) freedom to encounter others in relationships that are not merely determined by the needs of an advanced industrial state at the expense of autonomous individuals. This vision of emancipation is neither in the centralist revolutionary theory of Marxism-Leninism nor in the conventional theories of advanced industrial development. Thus, the theory provides a critique of varieties of 'vulgar' Marxist theories and the conventional theories of modernization.

The Frankfurt school, following Hegel, attempts to unite theory and action, and argues that revolutionary "praxis," action based on a truthful analysis of existing social conditions, is its goal. They accept the relativism of truth to a great extent, believing that truth grows out of specific cultural and historical circumstances and can only be tested and recognized through taking action, attempting social change. In addition, only action that is grounded in fruitful theory can have liberating results. Intellectuals working in repressive and alienating societies, societies not based on reason, cannot hope to foretell the future. Since men do not make their history in the current era, neither can they understand it or foresee the future. A good social scientist today is rooted in his culture, bounded by it, and only occasionally able to rise out of it enough to perceive the negative forces and tendencies in society that point to a new possibility in the making. It should be his goal to identify these forces and tendencies and clarify for his fellow man real social alternatives that are better than the existing order. Obviously the notion of "objective" social science, untainted by values or active participation is an impossibility, in fact anathema, to the Critical Theorists.

The Critical theory, as developed by Habermas, posits a very important role for the intellectual community. If intellectuals could attain "a self-consciousness of communication that is free from domination" then this model of rational discourse could be used for the "critique of power distortions of everyday communication." This ideal and historically specific model could then be used to analyze other social institutions. (Shroyer, 1973, p. 34).

According to Schroyer (p. 29) methodologically:

> "A critical theory begins from the position that a codified scientific method is not an adequate formulation for the validity of truth claims. Only in a reconstruction of the self-formation of developmental processes is there objectivity about the relation between these processes and our cultural understanding of them. Only by an explicit awareness of the dialectical reciprocity of history and culture can we attain an adequate grasp of the immediacy of the present. We approach 'social facts' from a cultural perspective which is not neutralized by research techniques since these may themselves conceal our prejudgments of the 'data.' Our social inquiry begins from and returns to the immediate and we must become aware of the influence of our cultural situation. This cannot be achieved by the development of a 'sociology of knowledge' but by the extension of the Hegelian-Marxist method of phenomenological reconstruction."

According to these theorists the task of critical reflection is not to provide the critique of existing knowledge but a critique of reified consciousness. By applying this method to the analysis of the industrially advanced societies, the Critical Theorists have been able to make the essential point "That the modern industrial world has severed theory from the practice of life" (Schroyer, ibid., p. 28). Some of the sources of the contemporary crisis have been conceptualized by the critical theorists like Husserl, Heidegger and Habermas in terms of "Objectivism," "technicity," and "instrumental rationality," respectively.

Specifically, proponents of the Critical Theory have analyzed in detail the structure and supports of authority in modern industrialized societies on men. Such analysis they believe will free men from the effects of these systems in the long run. Their analysis also shows that the crisis in modern industrial societies can also be seen in the greater and greater resources and efforts at rationalization that are needed for continued economic growth, international stability, and internal mass loyalty. By operating on the basis of the flawed logic of science and technology the modern industrial societies of today produce unnecessary needs. These needs in turn produce conditions needed to perpetuate undesirable social roles and institutions. The contradictions between advanced technology and ecology, high growth in GNP and poverty, cybernation and unemployment are some of the examples of thwarted technocratic rationality. The analysis of the modern industrial societies by the Critical Theorists is quite different from the analysis of these societies by the conventional theorists. The contrasting analyses of the contemporary world order by theorists working within the framework of various perspectives pose serious problems related to (1) the understanding of the dynamics of the present world order, and (2) the transmission of culture to future generations.

III

In summary then, we can say that the flow of social science knowledge across national boundaries is a fact of an interdependent world and there seems to be very little likelihood that this flow of social science can be stopped. The flow obviously has many positive aspects. However, that aspect of social science knowledge which was directly related to theories of development came under serious scrutiny in the nineteen-sixties and seventies. Proponents of theories of development and modernization that were developed in the Center nations hoped that social policies guided by them would bring about development in the newly independent states. But by 1964 the public and political leaders in the new nations came to realize that development models based upon the theories which originated in the advanced industrial nations were inadequate for their purposes. This realization was further reinforced through discovery by the social science community, both in industrially advanced as well as less advanced nations, that prevalent sociological theories of development were culturally and ideologically biased. This in turn became the basis for new theories of development which articulated the processes of underdevelopment and development in terms of dominance-dependence models. Concepts such as imperialism, colonialism and neo-colonialism, committed sociology, and the like emerged.

In the beginning much of the discussion was directed towards economic development and increases in the GNP which became one of the most sought for indicators of development. Later, it was realized that although economic development is important, it did not capture the spirit of genuine development which included such goals as social justice, equitable distributions of socially-produced goods and services, human rights, etc. Increases in GNP did not automatically reduce poverty, malnutrition, unnecessary dominance and repression. Both corporate planning and state socialist planning meant centralized control of production in order to create material well-being. Planned social change came to be seen by people as the authoritarian destruction of their power to interpret their own needs for human community and individual development. Social science knowledge was seen as an instrument of the powerful to be used for the control and manipulation of the weaker sections in both advanced and less advanced countries. This new consciousness created a crisis situation within the social science community which reflected the larger political, economic and social crisis at national and international levels. The result was a proliferation of challenges to theories that claimed to be based on the scientific models but in fact undermined scientific inquiry based on rational discourse. It was realized that advanced industrial civilization is faced with a recurrent crisis which creates a need for unnecessary social constraints. The tendencies inherent in this type of civilization are destructive to eco-systems and the survival of mankind.

The picture which emerges from the discussion of these theories is that social science knowledge can only be produced from a given point of view. A point of view then, lends itself to certain kinds of policy formulation, and, if implemented, to a specific type of social and economic development.

Social scientists employ a large number of theoretical orientations—Marxist, Social Darwinist, Structural-Functionalist, Symbolic-Interactionist, Demographic or Technological Determinist, or Psychological Reductionist, to identify some of the contemporary traditions. It is now realized openly that an orientation selected by a social scientist cannot be culturally and ideologically free. There are many statements based on complex philosophical arguments often made in favour of this stance in social science. These are: (1) Science itself is a part of culture; (2) It is always specific to a cultural community—a community in which social scientists are socialized; (3) Science is both relative in formulation and subjective in understanding; (4) There is no last word in any science; (5) All theories are only provisional, and are more or less adequate approximations; (6) Each of the theoretical orientations employed by social scientists forms part of the technological and linguistic system and is an expression which can only be relative and culturally bounded to the system in which it was created.

The question, then, is how can we use culturally bounded social science knowledge for the purpose of educating the younger generation in a manner that retains its critical function and does not become an instrument of indoctrination and propaganda in the hands of the powerful. More specifically, how should one as an educator of future educators go about appropriating sociological knowledge produced in one cultural and national setting to the people living in another cultural and national setting?

IV

In the previous section we concluded that the social sciences are generally relative and culturally-bounded to the system in which they are created. Therefore, teaching cross-culturally is problematic. In this section the merit of a critical approach in teaching sociology is discussed. Hopefully, this approach will overcome some of the biases in a teaching style which does not recognize the relative and culturally-bounded nature of social sciences.

Many sociologists have addressed themselves to the merit of a critical approach in teaching sociology (Gouldner, 1970; Hammond, 1964; Horowitz, 1968, 1969; Coulson and Riddell, 1970) and have made valuable suggestions. According to them a critical approach emphasizes the need for teaching students about the subjective processes by which a science gets done rather than how it is publically reported. It points out that sociology is not only a series of findings, but also a set of styles. It is sensitive to the process by which a person becomes a sociologist. The feeling of those who suggest a critical approach to teaching sociology perhaps is typically expressed in the following remarks made by Gouldner, (1963, pp. 51-52) in connection with an argument as to why even the first-year student needs a critical approach to what he/she is taught.

> "If we would teach students how science is made, really made rather than as publically reported, we cannot fail to expose them to the whole scientist by whom it is made, with all his gifts and blindness, with all his methods as well. To do otherwise is to usher in an era of spiritless technicians who will be no less lacking in understanding than they are in passion, and who will be useful only because they can be used."

It is further asserted that sociology is not a closed system of determinate laws, and there is really no considerable amount of agreement about most things. This feeling perhaps is typically expressed in the following ways:

> "... the purpose of sociology is not to build a closed system of determinate laws, but by scientific study, to enable people to become aware of some of the social reasons for the social problems that they experience, to be able to direct their attention to appropriate sorts of remedies for them. It can also help them to understand themselves better." (Coulson and Riddell, 1970, p. 15)

A non-critical approach, on the other hand, would disapprove of discussion of personal reflections with students and printing of the results of such reflections for consumption by the young and the general public. It emphasizes that "the rationality of the final sociological product is a consequence of the rationality of the method for realizing that product" (Horowitz, 1969, p. 9).

Insights offered by Gouldner, Coulson, Riddell and others make us believe that by using a critical style of teaching sociology it is possible to generate a culturally and nationally sensitized "global" or "transnational" sociological knowledge which will be relatively unbiased. This kind of sociological knowledge may lend itself to generalization at certain levels of society and culture.[8] Below, in order of importance, various steps involved in teaching sociology cross-culturally are suggested:

1. Introduce students in country B to critical writings by sociologists in country A on the sociology of country A.
2. Introduce students in country B to the critical writings on country A's sociology by sociologists in country B.
3. Introduce students in country B to the contribution of, if any, sociological knowledge generated in country A to the world pool of sociological knowledge. This can be done by following the suggestions made in the next three steps.
4. Introduce students in country B to critical writings on sociology by its own sociologists.
5. Introduce students in country B to critical writings on sociology of country B by sociologists of country A.
6. Finally, introduce students in country B to the contribution of, if any, country B's sociology to the world pool of sociological knowledge.

The above style of teaching assumes that the instructor has an adequate background in the sociology of sociology and other related areas, and also possesses skills and insights required for comparison and synthesis of intellectual ideas both at the micro and macro levels.

The following pages illustrate, how "American" sociology may be introduced to students in other countries by using writings in the area of sociology mostly by Americans. Within the context of the subject matter of this paper two ways of presenting material to students seems appropriate. First, one can take, for example, a particular variable in a particular theory of modernization developed in the United States and critically evaluate its uses for non-Western society. McClelland's "n" achievement or Parson's pattern variables are examples. Also, one may consider many contemporary theories of modernization developed in the United States and critically evaluate their use in other countries. Latin American dependency theory and the Frankfurt School's critical theory are instances. Some of the salient points of these two theories, along with those developed in the United States, have been discussed in the previous sections.

The second way is to introduce to students the basic questions raised in the area of sociology of sociology and also introduce to them literature describing and analyzing the institutional historical, political, economic and ideological constraints under which sociology as a discipline, occupation and profession developed in the United States and some other Western-European countries. Selected aspects chosen for discussion are: (1) Sociology of Sociology; (2) Sociologists' Uneasiness; (3) Sociology is a Heterogeneous Enterprise; (4) Problems of Making Sense; (5) Who are Sociologists and What Do They Do Under Group and Societal Pressures? (6) "Value Free" and "Value Laden" Controversy in Sociology and Among Sociologists.

1. Sociology of sociology focuses, in historical perspective, on the influences of sociological factors and social conditions on the kind of work

sociologists are doing. Explaining the discipline sociologically contributes to an understanding of the sociology of knowledge. The sociology of knowledge, as developed by Karl Mannheim (1936), calls our attention to the fact that the ideas and modes of thinking that may characterize a period or a given level of society are influenced by the social setting and that there are social factors—social factors that can be identified and studied systematically—that affect the acceptance, rejection, or promotion of ideas by groups in society. "By focusing on knowledge as an important topic for sociological investigation, Mannheim brought to the forefront the importance of the subjective dimension of social life. Not only are institutions and social structures which support and provide the framework for intellectual life and activity to be studied, but so are the persons, the individuals whose activities are part of and constitute that group, class, or collectively expressed thought. The historical and social formation of the thought of members of society becomes a matter of epistemological importance." (Psathas, 1973, p. 4). For a long time sociologists, especially Americans, did not make substantial attempts to identify, delineate, and analyze those social forces that shape and constrain sociological reasoning. The lack of interest among American and other sociologists in explaining the discipline sociologically undermines understanding and self-consciousness. "Unfortunately, sound sociological reasoning ceases precisely at that point where self-consciousness stops" (Reynolds and Reynolds, 1970, p. V, VI). Thus work in the area of sociology of sociology is directed towards increasing more self-conscious thought and research among sociologists and among those who read their work. Sociologists of sociology attempt to understand their own discipline's theories and practices by utilizing a sociological perspective.

For example, Tiryakian (1971; p. 5, 12, 13, 14) enumerates the following questions which sociologists of sociology may study:

a. Is the sociologist a sympathetic observer of his society? (p. 5)
b. Does he see himself as an academic critic of his society, and if so, what is singled out for criticism from the many facets of social life and what is not criticized? (p. 5)
c. Or does the sociologist see his role to be that of an activist, which in itself carries a broad spectrum of orientations, ranging from an active dissolver of existent social structure to one who takes upon himself the burdens of recreating a social order, a sociocrat in the Comteau tradition? (p. 5)
d. What social variables account for differentials in these orientations of sociologists to their societal setting? (p. 5)
e. How does a given theoretical orientation transform its applicability to society as it goes from one societal setting to another? For example, as Gurvitch briefly suggests, Marxist sociology has a critical and radical orientation in the West but in a communist state (as in Russia) it becomes ideologically supportive of the societal order? (p. 5)
f. Why and how do the values expressed or contained in some sociological theories or method become accepted over others, and under what conditions do they go out of favour? (p. 12)
g. What social factors account for some sociological group or school in a given country achieving intellectual dominance over contrasting groups or schools? (pp. 12, 13)
h. What are the critical resources and facilities of both the sociological profession and the larger society which make this possible? (p. 13)
i. Is it just a case of more forceful personalities or are there structural factors worth investigating? (p. 13)
j. What kind of training do we and ought we to give to students, undergraduates and graduates, that is, how is the sociology curriculum structured and towards what ends? (sociology for what?) (p. 13)
k. Who gets interested in majoring in sociology and in doing sociology as a life work? (p. 13)
l. What are some of the group characteristics of sociologists and how do they compare with other professionals? (p. 13)
m. What are the implications and consequences for the nature of the sociological endeavor when it is done by persons of different social backgrounds (in terms of class, ethnicity, race, religion, metropolitan/rural)? (p. 13)
n. How does the staffing of American sociology in terms of the social and intellectual backgrounds of its practitioners compare with other countries? (pp. 13, 14)
o. What are the major differences in the sociologization and recruiting processes of sociologists? (p. 14)
p. Is it by chance that most of the prominent figures today identified with the structural-functional school were graduate students of Harvard in the 1930's while most of the prominent 'conflict' theorist's did their advanced work at Columbia in the 1940's or 1950's? (p. 14)

q. More generally, what kinds of informal structures, relationships, and social networks exist among different groups in the profession? (p. 14)

r. On a comparative basis, where and under whose auspices is sociological research done, and what consequences stem from different structural organizations of sociology? (p. 14)

s. What accounts for shifts in departments considered pacesetters in sociology, or more crudely, for shifts 'where the action is' in sociology? (p. 14)

The sociology of sociology is a relatively new field of study in the United States. Perhaps, C.W. Mills (1959) was the first to bring a sense of self-consciousness to American sociology. Friedrichs (1970, p. 34) pointed out that in 1967 for the first time there was a session on the sociology of sociology at the annual meeting of the American Sociological Association, the official professional body. This was considered a breakthrough in the thinking of American sociologists. Since 1967 many books have been published which provide in historical perspective a comprehensive investigation of the influences of sociological factors and social conditions on the kind of work sociologists did and are doing. (Beal, 1969; Boalt, 1969; Friedrichs, 1970; Gouldner, 1970; Horowitz, 1968, 1969; Reynolds and Reynolds, 1970; Tiryakian, 1971). These sociologists have raised several other crucial questions that are relevant to the study of sociology from the perspective of the sociology of knowledge.

One may be more comfortable in reading sociological writings if they are approached with the awareness of the questions that have been raised by these sociologists. For example, the differences among the three theories discussed earlier become clearer when studied through the sociology of sociology approach.

2. Faced with the fact that others in society also claim to have knowledge that explains the functioning of sociology, sociologists find themselves in a situation in which they feel compelled to justify the superiority of the knowledge produced by using sociological methods. This is the source of their uneasiness. It will be of great help for those who read sociological studies to first understand the socio-political history of this justification. It is a fascinating history in itself in the sense that it reveals how sociology became an occupation, a profession, a scientific discipline, and how sociologists successfully justified their claim that sociological knowledge was superior to other kinds of knowledge about society. This history also reveals the political role played by those distinguished sociologists who lobbied for justification of sociological knowledge as the only legitimate knowledge that can be utilized for various social purposes.

It is not possible here to go into the exposition of this history. However, some factors that contributed and still contribute to the uneasiness of sociologists are mentioned here. This feeling arises out of the fact that during the last decade people of almost all disciplines have become interested in sociological studies. Sociology has become a popular subject for journalists, politicians, administrators, businessmen, school personnel, persons in mass media and the educated general public. One result of this popularity of sociology has been that most people tend to equate sociological insights with common sense. This attitude of the general public towards sociology makes "professional" sociologists uneasy.[9]

Related to the popularity factor are mundane considerations of making one's living and taking care of one's family. In a society where specialization of roles is the rule, sociologists cannot but feel the pressure of competition, the need to protect their specialty and jobs. For these and other reasons, sociologists like to see their discipline contributing to scientific knowledge about society. They also want to convince others in society that what they do is important, that they have special skills and knowledge to study society, and to produce needed knowledge. Further, they want to show that sociological knowledge is more useful and superior to common sense knowledge. Therefore, sociologists have worked very hard to make their discipline scientific—scientific in the sense of the natural sciences.

The interesting question is: Why like the natural sciences? Understanding of this aspect of making of sociology is important because without such awareness one is handicapped in fully comprehending why sociologists do what they do. "Today," writes Barnes (1972, p. 9), "after three centuries of exponential growth, science exists as a major institutional complex in all modern societies; its cultural and economic significance is now universally recognized and indeed, for some, it is the key to an understanding of the characteristic feature of 'advanced' capitalism."

Roszak (1973) writes that for the past three centuries science has dominated the development of Western Culture and society. In his words:

> "the mindscope to which our culture has been shaping itself over the past three centuries—and with ever more

> decisive urgency since the advent of industrialization—is the creation of modern science. Science, in turn, has reared itself on certain continuities of consciousness it inherits from the Judeo-Christian tradition of the West. What is important in the examination of people's mindscopes is not what they articulately know or say they believe. In that respect, our society is, at the popular level, all but scientifically illiterate. What matters is the taste that spontaneously discriminates between knowledge and fantasy. It is in all these respects that science has become the dominant force in designing the psychological and metaphysical basis of our politics." (p. XIX)

Hilgard and Lerner (1951, p. 38) have caught the essence of the drive on the part of American sociologists (and perhaps sociologists elsewhere) to become scientists when they note that:

> "the prestige of the natural scientists is high in American culture, where the developed industrial civilization and high standard of living are commonly attributed to scientific advances. This fact may lie behind the self-conscious wish of social scientists to become scientists like other natural scientists (chemists, physicians, biologists). Hence, those in the social disciplines turn to the natural sciences for their models of system building. They wish to attain the generally accepted criteria of good science: objective and reproducible observations; precise and valid instruments for refining observations; hypotheses that help to initiate inquiry and to direct research general theories; and laws that satisfy the esthetic demand for an articulated and harmonious system."

The flow of social science knowledge cross-nationally and its impact on the whole society or part of it cannot be fully assessed unless one is conscious of various socio-cultural factors, motives and necessities that influence the production, flow, diffusion, and institutionalization of social science knowledge. Therefore, it is logical that the social history of various theories of development and societies be read and taught first.

There is also a need to understand thoroughly the socio-political context which initiates the flow of social science knowledge cross-culturally. The picture becomes complex when it is realized that the flow of this kind of knowledge has been selective. That is, all aspects of social science knowledge which are produced in one country do not flow to other countries. Only selective aspects of the knowledge seem to get transacted. One needs to know at what level of society the transaction of this knowledge takes place, who is involved in the transaction, the specific duration of transaction, how the transacted knowledge is used, by whom or for what purposes it is used, on whom this knowledge is applied, and what are the consequences of the flow.

There are other questions related to the production and flow of social science knowledge. For example, what is the meaning of classified knowledge or information? Strategic knowledge? How does it come to be defined? To what degree is this proposition true?—social science knowledge that has potential use for propaganda and social control is more likely to be exchanged transnationally.

The problem of selection raises even more serious questions when discussed in the context of notions such as the free flow of information and censorship. Much debate is going on concerning the question of the right to communicate.[10] This issue is not discussed here, although it is an important one and is closely related to the exchange of social science knowledge and the teaching and learning of it.

3. Sociology is a heterogeneous enterprise. It is true, as mentioned earlier, that sociologists have a different image of sociology. But what is sociology from the perspective of sociologists? The heterogeneous nature of sociology, it will be seen, makes it difficult to answer this question. It also makes an understanding of the exchange of social science knowledge more complex.

The fact is that sociologists often fight among themselves on the definition, on the scope, and on the subject matter of sociology. Sociologists study a variety of social phenomena—race relations, poverty, crime, rape, homosexuality, war, revolution, peace, social change, family, prostitution, student culture, student power, alienation, and so on. A look at the complete program of the recent World Conference of Sociology may make one more familiar with the variety of areas in which sociologists do their research. There seems to be no end to the list of topics sociologists study. Moreover, not all sociologists study every topic or the same topics through the same perspective. There are various sociological perspectives as well as many sociologists. For example, there is an American sociology, German sociology, Indian sociology, Latin American sociology, Soviet sociology (see Tiryakian, 1971), African sociology (Mabugame, 1968), and Canadian sociology (Connor and Curtis, 1970).

Some sociologists prefer to talk about sociology as a unified discipline.[11] Thus, they prefer to say sociology in the United States, in the Soviet Union, in Canada, and so on. And yet others talk about academic, mainstream and establishment sociology (Gouldner, 1970), radical sociology (Colfax and Roach, 1971), new sociology (Horowitz, 1964), critical sociology (Birnbaum, 1971), 19th century sociology, sociology of thirties and forties, fifties and sixties, and so on. Others talk about black and white sociologies (Ladner, 1973), and bourgeoise and proletarian sociologists (Cornforth, 1972). One also hears sociologists talking about Western sociology, Third World sociology[12] and so on. Besides this, others talk about humanistic and scientific sociologies. This different terminology indicates that sociology is a very heterogeneous enterprise.

4. The diversity in sociology, then, raises the question: How to make sense out of sociology? It also raises questions regarding the creditbility and accountability of sociology. The meaning of these two notions cannot be grasped if they are isolated from socio-political-cultural dynamics. Those who teach and read sociology will be in a more advantageous position if they are aware of:

(1) the cultural roots of sociology, related both to the various approaches listed above and to linkage among approaches, and

(2) the notions of credibility and accountability not only at a national level but also at the level of cross-cultural exchanges.

In this connection, one of the important points which should be grasped is that the work of sociologists has the potential for impinging on people in a society. In this situation it becomes important that the public and the student of sociology understand the meaning of the work sociologists do. Sociological research is increasingly being used by those in power and authority for formulating social policies, and for enlightening and improving the public mind with respect to various social problems. Also, one needs to anticipate the impact of social research on social structure and social organization, for the very act of sociological research has the potential to change the existing reality. In this sense, sociological findings have become effective ammunition and are often used as such by various interest groups to achieve certain goals in the political arena.

Social scientists who conduct research on various social problems and then publish their research findings in selected journals, which have their own histories, are implicitly involved in political activity in the sense that their decision to publish has the intention of mediating the existing situation, although many of them may not be conscious of this or may flatly deny that their research might have any political consequences. But there is a need to keep in mind that as soon as the researcher imposes conceptual order on selected data his/her activity becomes a social activity with potential political implications and consequences.

5. If there are many sociologies as we have witnessed above, then there must be various sociologists who exhibit different life styles, sentiments and outlooks and who work at a variety of jobs at all levels of society. Those who encounter sociology should know about sociologists and what they do under group or social pressure. Reflection on these factors tells us in what way the production of sociological knowledge is very much dependent on the choice of questions sociologists raise or are able to raise under a given economic, political, social and cultural condition.

Coulson and Riddell (1970), commenting on the work of sociologists and other social scientists, make several interesting points: (a) sociologists make propositions about people, but sociologists themselves are people; (b) sociologists study a diversity of topics which involve them in great historical arguments—the arguments which arise directly out of human conditions which have affected the lives of a variety of people, and the arguments in which different people took part, for example famines, poverty, revolution, etc.; (c) because sociologists are people who live in society, they are also citizens with their own interests and views reflecting their own experience and perceptions; (d) sociologists as citizens find it difficult to separate their own views from their work; and (e) the decisions by sociologists to study certain topics as against other topics are social decisions.

Coulson and Riddell point out that since sociologists are members of a group, they are not free of group expectations. This situation poses several problems for sociologists since those who read their studies cannot grasp the meaning of sociologists' work unless they understand the sociologists' problem. Sociologist working in various social contexts are usually pressured to conduct their research within a certain framework. At the level of society "those groups who have effective control of a society may propose that only certain forms of approach or theory are proper, and may be prepared to enforce their view with violence or threats to the security of the sociologists" (Coulson and Riddell, 1970, p. 9). Sociologists in Fascist Germany (Neumann, 1966), and in Italy (Merrington, 1968) have faced such a situation. Moreover, control over sociologists may be exerted by more subtle means.[13] Referring to the study done by Birnbaum (1958), Coulson and Riddell write that ". . . Strong pressures may be exerted by those controlling societies, in some cases more directly, in some cases in a more roundabout way, for a sociology to be taught in a certain way, for some approaches to be emphasized at the expense of others" (p. 10). For example, in North America students are not introduced to Karl Marx's[14] social, political and philosophical thought in the same way as they are introduced to the work of Max Weber, Emile Durkheim, and the like.

In this regard it is important to mention that well known American sociologists themselves seem to have read Marx and other great sociologists like Weber, Durkheim, Simmel, Veblen, etc. rather carefully but have somehow failed to encourage their students to do the same. Strauss (1967) makes an interesting observation on the attitudes of some contemporary "master" sociologists. He points out that

> "currently students are trained to master great-man theories and to test them in small ways, but hardly to question the theory as a whole in terms of its positions or manner of generation. As a result many potentially creative students have limited themselves to puzzling out small problems bequeathed to them in big theories. A few men (like Parsons and Merton) have seen through this charismatic view of the great men sufficiently to generate 'grand' theories on their own. But even these few have lacked methods for generating theory from data, or at any rate have not written about their methods. They have played 'theoretical capitalist' to the mass of 'proletariat' testers, by training young sociologists to test their teachers' work but *not* to imitate it." (pp. 10, 11)

And to encourage the testers, writes Strauss (p. 11, footnote 10)

> "the 'theoretical' capitalist carefully writes his theory so it can be readily operationalized and proven wrong in several ways—a temptation for those who like to prove the theorist wrong. These proletariat testers do not realize that allowing themselves to be tempted simply puts the refined theory and the theorist on firmer ground, while (the testers) are soon forgotten."

6. We have already alluded to the fact of continuing dialogue among sociologists over the "subjective" and "objective" or the "value free" and "value laden" nature of sociology.[15] Here we shall further comment on this controversy. The notion of "subjectivity" is generally associated with "value" and the notion of "objectivity" with "value free." Further it is assumed that science is "value free," i.e. "objective." And if sociology is a science or ought to be a science, the argument goes, there is no place for "values" in the process of making sociology scientific—scientific in the sense of the natural sciences. Lundberg (1942, p. 53) expressed the paradigm of the value free social scientists most forcefully in the following words:

> "A common and less conscious source of error is the tendency of scientists, and especially of social scientists, to permit the current code of morals of the community or their own personal notions of ethics to influence them in their collection and manipulation of data. The findings of science are *per se* nonethical. It is not the business of a chemist who invents a high explosive to be influenced in his task *as a scientist*, by considerations as to whether his product will be used to blow up cathedrals or to build tunnels through the mountains. Nor is it the business *of* the social scientist, in arriving at laws of group behavior, to permit himself to be influenced by considerations of how his conclusions coincide with existing notions, or what the effects will be of his findings on the social order. For his purpose as a scientist, science must be an end in itself . . . As a scientist his only ethical responsibility lies in seeing that the rules of scientific procedure have been complied with."

This paradigm, as Horowitz points out, dominates some of the styles of sociologists and social scientists. This attitude towards science on the part of some sociologists leads them to believe in scientism (as an ideology) and not in science as a method of inquiry into society. Scientism and professionalism when fused together become a powerful force. The combination of the two makes it possible for some sociologists to occupy relatively important positions in the occupational structure, and perhaps because of it, leads them to become apologists for the status quo. Quoting Goode (1960, p. 902), Friedrichs (1970, p. 89) points out that besides Goode's opposition to the professionalization of sociology, he admitted that "No occupation . . . becomes a profession without a struggle . . . The emotion-laden identification of men with their occupation, their dependence on it for much of the daily meaning of their lives, causes them to defend it vigorously and to advance its cause where possible." Further, Friedrichs notes similar kinds of logic in Merton's (1962), p. 28) remarks and quotes him saying that ". . . as long as the focus of social power resides in any one institution other than science, and as long as scientists themselves are uncertain of their primary loyalty, their position becomes tenuous and uncertain." Friedrichs writes that "Merton and Stouffer had been among the most sophisticated exponents of ethical neutrality (i.e., value free stance in sociology). But in this instance (at the time when "the lobbying activity that went into the successful effort to cut the social sciences in on the NSF pie . . .") they were willing to go far beyond a simple denial of the prophetic stance to sell the behavioral sciences as guardians of American might in the confrontation with international communism." In order to make a case for the behavioral sciences in the United States they did not hesitate to use Cold War rhetoric implying the fear of communism. This is clear when one discovers in the pamphlet "National Support for Behavioural Science" (1958) under the topic "Nature of behavioral science," the following:

> "The United States finds itself in a world situation which demands assessment of every resource of physical, intellectual, and moral power. (But) . . . we could be surpassed by a country which concentrated serious effort to that purpose, if we do not intensify our own efforts. We must assume the probability of a breakthrough in the control of the attitudes and beliefs of human beings ... This could be a weapon of great power in Communist hands, unless comparable advances in the West produce effective counter-measures (pp. 5, 6 cited in Friedrichs, 1970, p. 88).

It was pointed out that social scientists have worked very hard to become like natural scientists and some of the reasons for this drive on the part of some sociologists were pointed out. The cloak of neutrality, to borrow the term from Friedrichs, or "scientific objectivity" or the "value free" stance or ethical neutrality—served well as a powerful ideology for many sociologists to fulfill their drive, especially when:

> "sociologists gradually became aware that identification with an image before which the public had already come to defer—that of the natural scientist—(it) was much the more prophetic step to take. And since the prophetic role had never been a tempting alternative to the physical or biological scientist it seemed equally inappropriate and dispensable to the sociologists. As training in sociology served increasingly as a tried and true vocational ladder for sons and daughters of the lower middle classes, it has become increasingly difficult for sociologists not to feel pressed by the immediacy of their professional interest." (Friedrichs, 1970, p. 89)

But Madge (1962, p. XXXII) has pointed out that:

> "in recent years there has tended to emerge a fresh point of view. This begins with the conclusion that pure value-free social science is unattainable, and that the inevitable bias of the investigator is less dangerous if fully exposed. But it has gone further than that, and it is now suggested that the social scientist *ought* to have a standpoint on social issues in order to ensure that he has 'an actively selective point of view'. It is claimed, by R.S. Lynd for example, that without this active concern with the point at issue the investigator will lapse into the random collection of pointless facts and will claim immunity from the need to organize these facts and to act upon them until some unattainable time when all the data are safely gathered in."

This is an important point to note. Many social scientists have developed the habit of investing countless variables in order to explain a particular phenomenon, for example, students' educational aspirations. They keep inventing "new" instruments to measure students' aspiration, keep using "new" statistical procedures and computers. And when the time comes for summarizing and interpreting data, it is said that the problem was complex, all variables could not be taken into account due to various reasons, and that further sophisticated research is needed. The key words are that the problem is complex and more research is needed. Thus the purpose is lost and as Horowitz (1968, p. 161) points out "specialized techniques of questionnaire design, codification, and compartmentalizing allow for the interviewing process to become the end of research rather than an instrumentality." In the same vein the editor of a UNESCO survey of political science indicates that:

> "a good deal of political research carried on in universities today is aloof from the real problems of political life. Too often research seems to be conducted for the sake of research. The topics chosen have no apparent significance, and the investigation does not throw light on any contemporary problem of importance. There is no driving force behind such research, no vital motives inspiring the work, no useful potentialities in the conclusions which emerge." (Robson, 1954, p. 116)

There is another tendency related to the gathering, analyzing and reporting of data. That is, many social scientists have developed the habit of consciously using esoteric and difficult language in reporting their findings. C.W. Mills (1959, chapter 2) has discussed this habit of sociologists at length. Sociology had become "less a creative ethos and manner of orientation than a set of Science Machines operated by technicians," wrote Mills (1959, p. 16).

That sociologists (social scientists, in general) need to adopt an actively and openly selective point of view has been suggested by several other sociologists besides Lynd. Martindale (1957, p. 367) points out that, in the study of social problems, value premises are necessary for significant and effective research. In discussing "the conflict of normative and empirical approaches," he concludes that theories of social problems are normative in nature rather than empirical; therefore they are outside the province of the scientist. And ". ... to seek scientific objectivity under conditions where it was in principle impossible," is nothing but a reflection of a scientist's misguided effort. Gouldner (1963) points out that the study of the human situation without valuation is not only of little significance both to the individual and society, but such study may also be impossible. Seeley (1963), Wilhelm (1964), and Backer (1967) make similar points. Thus many social scientists recognize the place of values in the study of social problems as both legitimate and methodologically sound

The point is that in the study of social problems there is no single perspective. The particular perspective selected by a sociologist reflects his personality, class, political orientation, academic training, and unique experience with the affairs of people in his own and other societies. The lenses through which he sees social reality are shaded by his perception of the whole world. However, this does not mean that a sociologist should feel astonished or ashamed of the fact that his own personal, cultural differences influence his selection of a perspective, or that his study will be biased by this fact. On the contrary, a social scientist who is studying other human beings should be conscious that he himself is a member of the same species. Basic human qualities are among the necessary tools in his field of work. The attempt to suspend these qualities is doomed to failure as research is a social act, intervening into and influencing society. A responsible researcher is aware of this and attempts to follow up and direct the consequences of his research. In this sense, as Seeley has emphasized, an act of research is never really finished. But even if it were possible to suspend one's humanity while engaging in social research, it is doubtful if it would yield desirable results. Truly disinterested research in the sense of research without evaluative "biases" would be research without purpose or relevance to the human condition.

NOTES

[1] Dr. A.J. Gunawardana, Senior Fellow, the Culture Learning Institute and Head of the Department of English, University of Sri Lanka, Vidyodaya Campus; Suranji Candappa at the Culture Learning Institute; Susan Singh, a free lance sociologist; and Dr. Richard Brislin read the earlier draft of this paper. I am grateful to all of them for providing valuable suggestions which helped in the reorganization of this paper and to Charlene Fujishige and Jo Ann Oda for their endless patience in typing this paper several times.

[2] The discussion in this section is a modified version of an earlier article of mine entitled "The New Economic World Order: The Social Context of Future Education." See the *Morning Watch*, Vol. 4, No. 1, 1976, Committee on Publication, Memorial University, St. John's, Newfoundland, Canada. The article contains a lengthy bibliography relevant to the topic of the new economic world order.

[3] The goals of genuine interdependence are linked with such issues as equality, distributive justice and the reduction of dominance in all spheres of social life. Briefly, interdependence among nations based upon equality and justice is a genuine interdependence. These goals are not new. Historically, theories about the division of labor, the origin of caste, class and hierarchy, and theories about the consequences of these, have essentially been concerned with the goals of genuine interdependency.

[4] I wish to acknowledge the help of Bhupendra K. Joshi, Department of Political Science, University of Hawaii, in developing this section of the paper.

[5] Two excellent books, one by Martin Jay and another by Trent Schroyer both dealing with the origin and development of the Critical Theory and a history of the Frankfurt School and the Institute of Social Research are now available. See Martin Jay, *The Dialectical Imagination*. Boston: Little, Brown and Company, 1973. For reference to Schroyer's book, see the bibliography. These books contain extensive bibliographies on the Critical Theory. Also see, Howard, Dick and Klare, Kail (eds.) *The Unknown Dimension: European Marxism Since Lenin*. New York: Basic Books, 1972. Sartre, Jean-Paul, *Search for a Method*. New York: Knopf, 1963. Garaudy, Roger, *The Alternative Future*. New York: Simon and Schuster, 1974. Althusser, L. For Marx, New York: Patheon, 1970, Habermas, J. *Legitimation Crisis*. Boston: Beacon Press, 1975.

[6] Latin American dependency theorists have worked in the areas of mass media, ideological implications of the behaviorist and scientistic methodologies and related problems of cultural dependence. The Frankfurt theorists have built their theories upon a long tradition in the philosophy of science stretching from Kant, Hegel, Marx and Weber to Lukacs. The interesting thing to note is that these theorists are reaching similar theoretical positions. See footnote 15 in Smith, Jr.'s article included in the bibliography.

[7] Discussion of the Critical Theory in this section is mainly based upon Schroyer's account of the theory (see introduction specifically, pp. 27-35). For the complete citation, see the bibliography.

[8] See Moore, W.E. "Global Sociology: The World as a Singular System," *The American Journal of Sociology*, Vol. LXXXI, No. 5, March 1966. In this article Moore notes a renewed quest for generality in sociology and to make it global. According to him "a powerful influence has been the course of contemporary history, which is divisive in important and conspicuous ways, but has elements of unity, such as the universal quest for economic growth and political participation . . . the methodological moral is drawn that our system must at times cut across national frontiers and, indeed, here and there, become global" (see the abstract).

[9] See Merton's remark on the uneasiness of sociologists in Friedrichs, R.W. (1970). Friedrichs explains: "In striving to build the case for his—and sociology's—new 'detachment', a New Yorker portrait of Merton reported him mumbling, 'that damned popular image again!' in response to a congressman's jab of 'Why don't you sociologists get to work and contribute something useful?' " (p. 88).

[10] See the background report for the UNESCO Intergovernmental Meeting of Experts to Prepare a Draft Declaration on Fundamental Principles Governing the Use of the Mass Media in Strengthening Peace and International Understanding and in Combatting War Propaganda, Racism and Apartheid (Com-75/conf. 201/4); UNESCO: Eighteenth General Conference. Report of Commission IV (18C/123); Jim Richstad (ed.), New Perspective in International Communication, East-West Communication Institute, Honolulu, 1977.

[11] In several professional meetings and private discussions I have witnessed sociologists making distinction between, for example, "Canadian Sociology" and "Sociology in Canada," Those who use the phrase "Canadian sociology" seem to emphasize the need to develop sociological theories responsive to the national and local interests. On the other hand, those who use the second phrase seem to be more interested in testing sociological hypotheses drawn from theories formulated by leading sociologists and are less interested in the significance of such sociological research for national or local problems.

[12] See Casanova, Pablo Gonzalez, *Internal Colonialism and National Development*. St. Louis: Washington University Monograph Series, *Studies in Comparative International Development*, 1, No. 4, 1965. Stavenhagen, Rololpho, "Classes, Colonialism and Acculturation," *Studies in Comparative International Development*, 1, No. 6, 1965. Stavenhagen, R., "Decolonizing Applied Social Sciences," *Human Organization*, 30, No. 4 Winter, 1971. Kruse, Harold, *Rebellion or Revolution*. New York: William Morrow and Co., Inc., 1968. all these articles deal with "the idea of internal colonialism. . . it became an important paradigm with respect to the oppression of Blacks, Chicanos and other Third World People." For further references and discussion see Blauner, R., and Wellman, D., *Towards the Decolonization of Social Research*, in Ladner, J.A. (ed.), see the bibliography.

[13] See Lazarsfeld, P. and Thieleus, W. *The Academic Mind*. Glencoe: The Free Press, 1955. These authors point out that the Social Science teachers in the United States were pressured in a roundabout way to teach their subject in a certain way by those who controlled the social studies establishment. The point that strong pressures may be exerted is also made by Birnbaum (1958).

[14] As late as 1962, Mills commented on the unwilling attitude of [illegible] nerican sociologists to recognize the contribution of Marx to [illegible] discipline of sociology. However, after several years of [illegible] on the part of some sociologists with "the conspiracy of [illegible] about Marx's social theory many sociologists have [illegible]med the re-discovery of Marx in North America and other countries during the last decade or so. Well known sociologists and social psychologists in America now recognize the contribution of Marx to the development of various aspects of sociology. For a comprehensive discussion of Marx's influence on sociological thought in the United States and elsewhere see Chapter 11 entitled "*The Marxist Analogue*," pp. 259-287, Friedrichs (1970).

[15] Friedrichs' book also contains extensive discussion and bibliography on the "subjective," "objective" controversy in sociology. See Chapter 4 entitled "*The Cloak of Neutrality*," pp. 77-91, and Chapters 5, 6, and 7.

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PONAPE: CROSS-CULTURAL CONTACT, FORMAL SCHOOLING, AND FOREIGN DOMINANCE IN MICRONESIA

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SETTING

Northeast of New Guinea and about 6°.14′ above the equator lies Ponape, a high volcanic island in the Eastern Caroline Islands. Ethnographically, Ponape is part of the greater cultural area known as Micronesia or 'tiny islands'. But as Mason (1968) suggests, the concept of Micronesia as a homogeneous cultural area, bordering Melanesia on the south and Polynesia on the east, loses its utility when confronted with the vast heterogeneity of cultures and languages within its bounds.

Politically, Ponape is the administrative center of one of the six districts of the United Nations Trust Territory of the Pacific Islands. The other districts are Palau, Yap, Truk, the Marianas, and the Marshalls. The Territory is administered by the Department of the Interior, United States Government. The Ponapeans are predominantly an agrarian people and tend to live on dispersed agricultural plots rather than in consolidated villages or in the district center enclave, although government employment and schooling have encouraged their steady gravitation toward the latter. The physical environment of the island, with its geographic isolation, high rainfall, and fertile coastal plains has structured the evolution of a subsistence agricultural economy functioning on an extended communal family organization as a means of assuring survival.

THE SOCIO-HISTORICAL PROCESS

For centuries, the knowledge, skills, attitudes, and beliefs commensurate with a subsistence agrarian economy have been transmitted informally in the situations of daily Ponapean life. However, throughout the so-called "Age of Exploration" of the sixteenth century and the more recent "Era of Modern Imperialism" of the nineteenth and twentieth centuries, cross-cultural contact and the subsequent imposition of alien ideology and technology have come to bear heavily upon this indigenous mode of cultural transmission. The schools have evolved as the central institution for alien cultural intervention in Ponape. In short, the indigenous forces of enculturation have been challenged by the alien acculturative phenomenon of schooling. Schools have become the primary instrument of foreign dominance and control.

As Ballendorf (1968: 1) noted, "There have been four alien administrations, each bringing to the Micronesians its own brand of development and colonialism; each leaving a semblance of its own culture which has been absorbed in varying degrees." In all of these administrations (Spanish, German, Japanese and American), schooling has played a significant role.

Even before the first Western contact period in Ponapean history, legends state that during the migrations from the Asian sub-continent through the Pacific, famous houses of learning ("primitive" universities) existed in New Zealand and the Society Islands. In these houses of learning priests taught ancestral lore, genealogies, religion, magic, navigation, agriculture, arts and crafts (Luomala, 1946). Whether Ponapeans participated in any of these formal educational institutions is unknown. That Ponapeans had their own indigenous educational processes often goes unnoticed.

It is clear that an indigenous, informal educational process had deep roots in Ponape long before the advent of schooling. This educational process differed greatly from Western schooling. Here, and in situations less clearly defined but much more common to the daily life of Ponapeans, knowledge, values, and skills were continually transmitted and renewed incidentally by word and by example. All education in this context was a direct response to social demands. Knowledge was sought where it was thought to be meaningful and a useful guide to one's survival. Childhood play, identifying and mimicking adult roles, became a lucid learning process sanctioned and guided by the community. Skills were learned by working side-by-side with elders as the prolonged observation and guided practice of childhood got its full test when learning proceeded from play and private trial to the public performance of survival tasks.

Attitudes, values and beliefs were often implicitly transmitted through Ponapean sayings and proverbs. Such maxims as "The quietness of a man is like the fierceness of a barracuda" conveyed moral lessons and social attitudes. Knowledge and skills common to daily survival were free for the observing, but matters dealing with magic, medicine, ritual, and legend were more difficult to acquire. It is believed that these areas were highly sacred and were to be passed on to select individuals within a specific clan. Different clans controlled different areas of knowledge which was often testimony to the clan's status. Matters of right relations or interpersonal gestures of respect behavior were usually demonstrated in the home and in the feast house, where parents were quick to point out appropriate behavior toward persons of differing rank.

The feast was the most comprehensive educational experience continually repeated before the eyes of the entire community, for here rituals were performed, songs sung, dances transpired, legends told around the kava stone, and special foods prepared, all in an atmosphere imbued with the acknowledgement of rank, status, and prestige. In this setting, the inculcation of skills, moral teachings, and attitudes of respect culminated to reinforce ritualistically the social solidarity of the group. The traditional feast can be somewhat envisioned as the nearest functional analogy to the modern day formal school in its emphasis on instruction, indoctrination, and social selection.

The major objective of Ponapean indigenous education was to inculcate the values of the group and to perpetuate these values through the linkage of past tradition with present action and future generations. Ponapean indigenous education was tradition bound and conservative. It educated to reproduce itself. Lineage became not only a biological construct but also an important educational concept inasmuch as the family was a central educational institution of the society. Education was consensus rather than conflict oriented. Social harmony and cultural continuity were the overriding themes. Although it may have tended to such educational activities as skill training and mental discipline, its primary purpose was to enhance social solidarity—to preserve and transmit the culture as it existed (Colletta, 1975).

As early as 1595, Quiros, a Spanish explorer, recorded the first sighting of Ponape by a Westerner, though it may also have been sighted in 1529 by Saavedra, who sailed from Truk to Kusaie. In 1827, O'Connell was shipwrecked on Ponape for a period of five years during which he worked as a mediator between the Ponapean chiefs and foreign visitors (Hambruch, 1932; Reisenberg, 1974). Numerous explorers, whalers, and drifters made contact with the Ponapeans during the 1800's but there is no evidence of the introduction of formal Western education prior to the first missionary effort in the latter part of the century. Hambruch (1932:74), in discussing the influence of early drifter and whaler inhabitants on Ponape observed:

> Through the influence the whites have obtained over the natives by speaking their language fluently and by adapting themselves as far as possible to their customs, the character of the natives has been very spoilt. They taught them how to make alcoholic drinks from the juices of the coconut palm, and have got them into the habit of lying and stealing. If these fellows are not removed from the island soon, they will have gotten the natives accustomed to all kinds of vices and immorality.

A Boston Congregationalist organization, which had already christianized Hawaii, arrived on Ponape in 1852. Hambruch's concern for the eradication of the "wicked" whalers was soon to be realized. The Ponapeans increasingly trusted the Protestant missionaries because they taught crafts, domestic science, and introduced new varieties of useful plants (Bascom, 1950). A girls' boarding school was begun for five students by an American woman missionary. Emphasis was on religious instruction. Work, however, was stopped abruptly in the late 1880's when trouble developed between the Spanish authorities and the American mission, leading to political intrigue, native rebellion, and the eventual banishment of the Boston Mission (Smith, 1968).

The missionary efforts at formal education during the Boston era and throughout the Spanish and German periods of hegemony were primarily concerned with spreading Christianity and "civilizing the heathen." Paramount to their work was the training of pastors, the teaching of basic literacy skills, and the translation and propagation of the Bible. Formal classes were arranged and conducted with small groups of Ponapeans in an atempt to meet these objectives.

The Reverend R. W. Logan made the following comment on such mission education:

> Forty-eight scholars can read, and it was a joy to see the whole number stand up and read, each from his own copy of the scriptures; many of them are getting beyond the stage of word-calling into that of intelligent reading. The school also did creditably in singing, writing, and the beginnings of arithmetic and geography (Bliss, 1906:68).

The irony of the 'altruistic' efforts of these early missionaries was that in saving them from a life of sin they contradicted some of the core values of Ponapean life. As Smith (1968:286) remarked, "The natives were to become Christians, and their lives were to be patterned after Western behavior, which was totally unsuited to Pacific island life."

The early missionaries not only interfered with the Ponapean feasting merriment and the important ritual of kava drinking by labelling it sinful, but also transformed the basic indigenous educational technique from a learner to a teacher (preacher) orientation as attention was directed toward finding neophytes rather than apprenticing oneself to an elder as in the indigenous system. Coupled with their self-righteousness was the gradual inducement of a personal guilt orientation to vie with the traditional Ponapean notion of shame as a major mechanism of social control.

Religious propagation and conversion were hardly detached from the later more general colonial economic objectives. As Hezel (1975:128) noted, "Religious instruction was the heart of the curriculum, but it was almost always supplemented with training in the trades (agriculture and carpentry, in particular)." Such moral attributes as self-discipline, duty, and penitence further served to enhance the social-psychological grip the expatriate warders held on the Ponapeans. Moreover by teaching the natives that the acquisition of a foreign language could be profitable, the mission schools also demonstrated to the ruling colonial government officials that bilingual natives could make profitable middlemen, paving the way for the secularization of schooling and the economic exploitation of human as well as natural resources.

During the nineteenth century the island of Micronesia became involved in European colonial rivalries. Both Spain and Germany had vested interest in the area. The Spanish were preoccupied with the propagation of Catholicism and international prestige, while the Germans desired the lucrative copra and shell resources. Near confrontation between these colonial powers resulted in Pope Leo XIII mediating the situation in 1888. The Pope confirmed Spain's claim to the Carolines but allowed Germany the right to continue fishing and trade, and to establish coaling stations in the area. In 1899, with the weakening of the Spanish empire and the loss of the Spanish-American War, Spain made the decision to withdraw from the Pacific entirely, selling the Carolines (including Ponape) and the Marianas (except Guam) to Germany for 25,000,000 pesetas (Ballendorf, 1968).

During the Spanish and German reigns, from 1885 to 1914, little official attention was paid to formally educating the Ponapeans. This was again left to the missionaries (Protestant and Catholic). The Spanish merely occupied the islands, protecting the honor of Church and State. The Germans were primarily concerned with trade, especially the development of copra. Only in the Mariana islands did the German government manifest interest in erecting formal schools. Although the Germans drafted plans for the introduction of formal secular education on Ponape the idea was quickly abandoned when World War I broke out.

Formal education on Ponape, prior to the Japanese occupation, consisted essentially of instruction in the language of the foreigners, some skill training, and the teaching of religious creeds of American, Spanish, and German missionaries. Training of educators was along ecclesiastical rather than pedagogical lines. As Hezel (1975:128) summarized:

> The emphasis may have differed (from the Spanish) under the short period of German reign, but the sacred educational triad still appears—the language of the foreign power, vocational skills, and moral training in those values that were most integral to the national spirit of the foreign power (although not necessarily of the native population). The overall educational program was fashioned so as to supply what, in the judgment of the colonial administration, was most needed to insure the progress of the native peoples towards civilization.

While the missionary schools under Spanish and German control preached Christian virtues, the governing colonialists themselves began to envision the potential of secular schooling for political indoctrination and economic exploitation.

In October 1914, the Japanese Navy took military possession of the Marshalls, the Carolines, and Northern Mariana Islands. As a result of the 1919 Versailles Peace Conference concluding World War I, Japan was awarded her occupied Micronesian territory as a Class C Mandate under the League of Nations. They were charged with the promotion of material and moral well-being, and the social welfare of the islanders (Clyde, 1935). In order to clearly understand the Japanese educational program one has to look first at the general colonial policy of the Japanese. Ballendorf (1968:6) summarized it as follows:

1. To develop the islands economically.
2. To prepare them for Japanese emigration, thus relieving population pressure in Japan.
3. To Japanize the natives as quickly as possible through education, propaganda, inter-marriage, and by promoting cultural exchange.
4. To fortify the islands in preparation for war in the Pacific.

A formal education system was rapidly installed in an attempt to realize the above goals, especially the "Nipponization" of the native populace. As Fischer (1961:524) further acknowledged, these goals were directed at eventual, "politico-economic integration into the Japanese Empire and the advancement of the natives by civilizing them."

During the Japanese reign a dual school system was inaugurated. Children of Japanese nationals attended "primary" schools modeled on those of the homeland, while Ponapeans attended "public" schools established for assimilation. A public school was defined as:

> ... an institution in which common education is given to native children, its fundamental objective being the bestowal on them of moral education as well as of such knowledge and capabilities as are indispensible to the advancement and improvement of their lives, attention being simultaneously paid to their physical development ... (Antilla, 1965:221).

Informants state that the schools stressed the Japanese oral language (approximately one half the curriculum), basic arithmetic, ethics, manual arts, and other subjects designed to supplement and support the Japanese economic endeavors. The program consisted of three years of elementary school, reaching fifty percent of the eight to fourteen year old Ponapeans with a small percentage of the remaining children attending the few mission schools. At the discretion of the local education officials, an additional two year course of study above elementary level might be offered at the district administrative center. Beyond that, a few students might aspire to the highest education in the mandate, the Woodworkers Apprentice School on Koror, Palau (Yanaihara, 1940). The extremely large number of Japanese colonists on Ponape (at one point outnumbering the native population), and their tremendous economic program included a multitude of non-formal and incidental educational experiences to which the native population was peripherally exposed. A sugar refinery; button making from ivory nuts; salted fish processing; the construction of a narrow gage railway and all weather roads, among other technological advancements, were introduced by the Japanese and further served as acculturative institutions.

During his research in the islands, Yanaihara (1940:245) found much to criticize in the Japanese education system. He observed that, "The use of Japanese teachers who had no idea of the mental capacity of Ponapean children, nor their habits and customs, coupled with the overemphasis on Japanese language in the curriculum seemed to disregard the special needs and requirements of the indigenous student." While attempting to evaluate the functionality of this type of education, he collected the following response from a Japanese public school teacher in Kitti, Ponape:

> The education is not of much practical value to the student. But the ability of the student to converse in Japanese is no doubt a concrete asset. The graduates are able to come into closer contact with the Japanese because of their knowledge of the language; their standard of living may be said to be progressing in comparison with that of the uneducated native, although the progress is only in a material sense, for it is mostly manifested in an improvement in food, clothes, household equipment and dwellings. Their moral progress is but slight. They also become smart and sharp, which is not always desirable. But this cannot be helped in a period of transition such as the present.

Continuing his criticism of Japanese education, Yanaihara (1940:247) found, "The present educational system does not take into consideration the practical side of native life and is likely to make the student a mere highbrow among his people. It must, however, be admitted that education is playing the most important role in modernizing native society and in conferring on the islander that adaptability without which he will not be able to survive the changed conditions in which he finds himself today." Japanese education for the Ponapean was highly moral and narrowly utilitarian, adjusted to meet Japan's economic and political needs, and like the prior mission schools often "civilizing" at the expense of traditional Ponapean life ways.

Many informants emphasized the extreme regimentation of the Japanese schools. A typical day went as follows: Up at dawn for the long walk to school as there were only five public schools on the entire island. Attendance was made mandatory by the Japanese police who were sent to punish parents by beating or public abuse for their children's absence. Once at school, all the students lined up at attention for roll call, raised the Japanese flag, bowed to the North in honor of the Emperor and sang the Japanese national anthem. Following this was a brief period of physical exercise. Upon entering the school the entire class stood to recite the rules in harmonic fashion as follows,"Respect your elders; don't steal, lie or cheat; pay attention in class; don't be tardy, etc." The class then proceeded into language drill, followed by arithmetic and geography. As Fischer (1961:519) recorded, instruction took the following course:

> The Japanese method of instruction relied heavily on drill and rote learning. The first item in an official curriculum description of arithmetic for first grade, for instance is "Recitation and writing of the numbers up to 100."; for the second grade the corresponding item is the same for numbers up to 1000. For subjects most emphasized, i.e., language and arithmetic, constant drill was probably especially efficient in the early stages of mass classroom instruction.

Discipline was extremely rigid with incidents of physical beating being very common. A student stood at attention while addressing the teacher. At report time parents were invited into the school to publicly share their children's rating.

The stress on rote learning, the authoritarian structure of the classroom, rigid discipline, and the exercise of public shame ironically paralleled the traditional Ponapean means of controlling and modifying behavior, and led to much success in the teaching of Japanese language and math skills. When asked to compare the Japanese schools with the present day American schools, the adult Ponapeans, although quick to recognize the limited opportunity for furthering education under the Japanese system as compared to the American, often openly admitted their preference for the rigid control and harsh teaching tactics employed by the Japanese. The Japanese school system in many ways reinforced the traditional values and methods of Ponapean enculturation, and was rapidly adapted to by the children and condoned by the adults as well.[2]

In at least one critical manner both the Japanese and the Missionary educational approaches were similar in their support of the traditional Ponapean social structure. They both used rote learning as a method of knowledge transmission, enchancing the indigenous style of learning through an emphasis on memorization and direct imitation. Although the missionaries introduced a personalized guilt orientation as a means of social control, the Japanese reestablished the indigenous emphasis on public shame. None of the aliens, whether missionaries, Germans, Spanish, or Japanese allowed the Ponapeans to fully participate in the foreign life space. Kolonia, the district capital, was virtually off limits to Ponapeans during Japanese times. This physical separation somewhat reduced the potential incidental impact of Japanese economic and technological innovation on Ponapean life ways, which although long in time duration was short on intensity. As G.P. Murdock (1948:28) reported at the close of World War II:

> ... The local cultures are everywhere still functional and still essentially aboriginal in character ... the old subsistence agriculture has not been destroyed.

In distillation, the most significant negative effect that early missionary and Japanese schooling had on the Ponapeans was to provide them with a cultural mirror which reflected their inferior status in the overall contact relationship. So began the subtle, often unconscious, process of learning one's inferiority through compulsory participation in foreign schools. As we shall see, this process was to be greatly advanced under the present American hegemony.

World War II activities, including the bombing of the District Center of Kolonia, led to the termination of the Japanese school program as the Ponapeans were rapidly assembled to assist in the defense of the island. This period was marked by forced labor and other maltreatment of the Ponapeans by the Japanese military, and produced bitter feelings that older Ponapeans still retain. With the Japanese surrender in 1945, the American Navy took charge of the islands and repatriated all Japanese in an effort to wipe out any remnants of their occupation. In many instances cross-cultural marriages were ignored and families were broken up. Such was the ominous beginning of the American occupation.

The American era can be divided into the period of Naval rule and that of Civil rule (the Department of Defense and the Department of the Interior). The U.S. Navy occupied the island from 1944 to 1947 and administered it in a very loose manner, sometimes referred to as the "zoo keeper" theory of control, with little initial effort of sustained institutional contact being made. During the early stages of Naval occupation every effort was made to preserve as much of the native culture as possible. Wright (1974:96), the first deputy high commissioner expressed the naval policy towards education in the following passage:

> I would give them schools with native teachers, trained not to impose an alien culture upon their charges, but to perfect their own ... That sort of education will not disturb their social, economic or aesthetic standards, which are adequate to them and in many ways enviable to us.

In 1947, the Trust Territory of the Pacific Islands, under the United Nations Charter and the United States Administration, officially came into being. The Naval administration then began to erect a system of elementary and intermediate schools established with the stated objective:

> To benefit the many and assure progressive development to each community along lines designed to raise the standards of food production and the nature of the food supply and to equip the local inhabitants for the conduct of their government and the management of their trade and industry (Department of Navy, 1950:1).

This seemed clearly at odds with Wright's earlier proclamation.

Little was accomplished until 1951 when the above charge passed from the Department of Defense to the Department of the Interior by executive order number 10265. The United States educational policy under the U.N. Charter agreement was as follows:

> ... To promote the educational advancement of the inhabitants and to this end shall take steps toward the establishment of a general system of elementary education; facilitate the vocational and cultural advancement of the population; and shall encourage qualified students to pursue higher education, including training on the professional level ... (United Nations Trustee Agreement, Article VI, 1948).

To these ends a school program was designed on the basis of a six-year elementary school for all; a three-year intermediate school for a selected few in each district; and one territory high school for the "especially capable" intermediate school graduates. During this maiden period of American administration, progress was very slow, marked by, in Ballendorf's (1968:9) words, "low budgets, mixed with a less-than-vigorous administration."

In 1961-62 under the Kennedy Administration, drastic changes in policy occurred in the American Trust Territory Administration: budgets were increased; English became the official medium of instruction in all the schools; a large number of educational personnel, both Contract and Peace Corps volunteers, were imported from the United States; and school construction boomed. Today, Ponape island alone has over twenty-six public elementary schools, one public high school, one teacher training institute, one community college, and numerous non-formal educational programs.

Recent missionary efforts have taken a secular bent, resulting in increased vocational and academic training complementary to the previous religious preponderance (McGarry et. al., 1972). There are now four Catholic elementary schools and one Catholic Trust Territory-wide vocational high school on Ponape. Other American based religious groups, such as the Seventh Day Adventists, have also begun missionary educational establishments on the island.

In sum, there is little doubt that the current educational efforts have taken on a distinct American flavor.[3] As Hezel (1975:130) noted:

> Democratic participation in society, an openness to the world-at-large, and entrance into the dollar economy imply certain values that are fundamental to a people. And it is these values — especially those of freedom of choice and egalitarianism— that lie at the bedrock of the American civitas.

Although the implication of the preceding is that most, if not all, of the impact of schooling on the native population was negative, in fairness it should be pointed out that schooling did provide limited upward mobility for some. It also promoted egalitarianism rather than the former unequal relationships of a feudal society. Finally schooling provided an expanded world view inclusive of an international language. However, in the balance, the impact of schooling on the Ponapean has not proven to be advantageous.

THE NATURE AND SCOPE OF THE CONTACT RELATIONSHIP

Much of the historical contact between aliens and Ponapeans has revolved around the central themes of commerce, warfare, colonization, and conversion. Ancillary to these forms of contact have been various modes of education. From the time of the early explorers, whalers, and missionaries, through the more formal Spanish, German, and Japanese hegemony, to the present American Trusteeship, foreigners have been concerned with "educating the natives." This process has implied, not only the imparting of literary and technical skills, but also changing the indigenous culture and character from that of "heathen" to Christian, from feudalist to petty capitalist, and from an authoritarian to egalitarian orientation.

The early missionaries, both Bostonian Protestants and Spanish Catholics, sought conversion and domestication of the Ponapean. The German regime desired economic exploitation of the island, while the Japanese strove for both economic exploitation and assimilation into an empire. In each instance education was utilized to control human and natural resources, to allocate social position, and ultimately, to define reality for the subordinate Ponapeans. The Ponapeans were impressed by the material and technological accomplishments of their foreign warders, and quickly sought to obtain them. "With something of the spirit of the cargo cult devotees, they look to the school as the quasi-magical means of introducing the millenary age of material prosperity into their society" (Hezel, 1975:130). Indigenous education through the extended kinship unit was no longer sufficient to meet the new material aspiration modeled in the person and goods of the foreigners. The new needs demanded participation in the alien institution of the formal school. Schooling fast became the new religion.

The predominant relationship in this meeting of cultures has been one of superordinate to subordinate. The foreigners have traditionally played the role of superordinate power, while the Ponapeans have performed the passive part of subordinate host. In characteristic colonial fashion, conscious and unconscious acts of generosity and patronage served to make the indigenous population more dependent on largess and less sure of their own inherent capabilities to produce.

Policies of parity and integration, although voiced by some of the warders, usually resulted in subtle domestication and assimilation of the Ponapeans. The initial potentialities of the contact situation, in terms of bi-cultural or trans-cultural evolution, more often than not ended in deculturation and personal marginality among many of the Ponapeans and concomitant ego-enhancement for the aliens. It was rare, indeed, if the colonial power took account of the indigenous context before embarking on a course of innovation cloaked in such terminology as "progress," "development," or "modernization." The social obligation and continuity that formed the core of indigenous education soon became social alienation and discontinuity for many under the colonial educational system.

From 'subjective' beings in interaction with their physical environment, the Ponapeans have been 'objectified' and handed down as human possessions from colonial warder to warder. They have gradually succumbed to the process of objectification by passively emulating their alien warders, not through first-hand participation as equals, but through vicarious observation and unquestioning assimilation of the alien mores over time. Without doubt, the Ponapeans actively sought trade with whalers, and industrial products there attached, long before any coercion from foreign governments was applied. Nevertheless, they quickly became subordinate even in these consciously desired relationships when eventually faced with the overpowering technology of the foreigner.

Created to institutionalize the contact relationship, giving it social legitimacy and structured assurance of survival, the school is the main technological innovation of colonial powers. As a "technique," schooling goes beyond the limited "hardware" concept to a broader definition of technology. Ultimately, it is not only a style of organizing and controlling behavior, but also a method of instilling an entire way of thinking and perceiving the world. As the Ponapean extended family and indigenous patterns of enculturation acted to insure the internalization of generational relationships and a unique way of looking at the world, so, too, did the introduction of formal schooling serve colonial regimes by establishing new relationships and inducing a new world view among the indigenous peoples. As Sol Tax (1946:337) so aptly observed, the central educational question throughout the history of cross-cultural encounters remains, "Whose cultural tradition is to be transmitted?"

Social, economic, and political expediencies share in the way formal schools have evolved in the contact situation. A biased sense of history is brought forth in order to induce inferiority and to provide a "scientific" rationale for the colonial relationship. As W.E.B. DuBois (1946:318) remarked:

> Ability, self-assertion and resentment among oppressed peoples must always be represented as irrational efforts of 'agitators,' folks trying to attain that for which they were not by nature fitted.

A review of historical contact will reveal that the control of access to learning and knowledge is the most certain way of holding the mass of people in a position of servitude and maintaining a given power relationship. While the delicate selection of a minority of host country nationals for advanced education more often than not leads to their conversion to the colonial way of life, the formation of a core of bilingual middlemen for the warders, and the deprivation of the masses of their natural leaders. Schooling has been used to inculcate obedience and servility lest the whole colonial relationship be overthrown (Turner, 1946).

In demonstrating the connection between formal schooling and economic development in South East Asia, J.S. Furnivall (1956:383) observed that, "Education, or rather instruction, is not preparation for the business of life, but preparation for a life of business." As agencies for incidental and informal education become incapable of training men for their increasingly complex environment, society gathers the children from learning in natural situations and assigns them to the schools. The increasing differentiation of developing societies through the introduction of the Western modes of subsistence leads to an increased division of labour and the formation of self-interested propertied classes. The schools have manifestly taken up the task of training the masses in the new skills while latently functioning as a mechanism for sorting and selecting the general populace into socio-economic classes.

The significant turning point in the cross-cultural contact relationship is the implementation of the technology of formal schooling as a major means of institutionalized control via the process of secondary socialization (acculturation) and occupational role allocation (social class formation.)[4]

Through influence, coercion and instrumentalism, the Ponapeans historically relinquished their political control to foreign warders and subsequently surrendered control of the education of their children. The process of education was gradually transformed from one in which the Ponapeans had full control of the continuous enculturation of skills, values, and identity into an acculturative, alien phenomenon, centered about the technological innovation of the formal school, marked by rampant change and cultural discontinuity.

The part played by the present American school system in the historical process of cultural contact and foreign dominance on Ponape has yet to be fully realized. Only time will give the perspective necessary to evaluate the effect of American schools throughout Micronesia. All indications from the author's recent study on Ponape are that the comprehensiveness of the American *compulsory* school system, in terms of percentage of people in school and years in attendance, is swaying the absorption process in favor of the alien culture. That schooling is a crucial denominator in the tenuous balance of cultures in contact was summarized years ago in Charles S. Johnson's (1943:632) remarks in a symposium on education and the cultural process:

> When peoples of different cultures come together, there is acculturation in which there transpires a constant struggle between disintegration and integration. Basically this is education. Education, thus, is more than the transmission of culture from one generation to another. It is this transmission and it is also the transformation of peoples who are more or less in conflict.

NOTES

[1] This article is derived from the data gathered in a larger study conducted by the author on the role of education in cultural change among the Ponapeans. The research methodology employed was primarily archival and ethnographic (participant observations and in-depth interviewing of key informants). While the investigator served as a Peace Corps Volunteer on Ponape from 1968-1970, the period of intensive study took place during the period January to June 1972. The study was funded by the Ford Foundation and would have been impossible without their generous assistance.

[2] The Japanese hierarchical social structure with its stress on rank (place); reciprocal obligation; respect for the wisdom of the aged; face or shame, and corporal mechanisms of control; and indulgent, dependence producing, patterns of early child rearing as evidenced in the work of Ruth Benedict (*The Chrysanthemum and the Sword*, Houghton Mifflin Company, 1946: Boston) is ironically very similar to indigenous Ponapean social structure and process.

[3] For an elaboration of the interplay between the method and content of the present American Schools and the Ponapean Culture see Colletta, Nat J. "Cross-Cultural Transactions in Ponapean Elementary Classrooms," *Journal of Research and Development in Education*, Volume 9, No. 4, 1976, pp. 114-123 and "PICS: A Case Study of Bureaucratic Organization and Cross-Cultural Conflict in a Micronesian High School," *International Review of Education*, Vol. XX, No. 2, Winter 1974, pp. 178-200.

[4] The recent economic historical work of Martin Carnoy, *Education as Cultural Imperialism*, John McKay Publishers, 1974: New York, and Herbert Gintis and Samuel Bowles, *Schooling in Capitalist America*, Basic Books, 1975: New York, tend to concur with many of the points made here within a worldwide context.

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EFFECTS OF MOTIVATIONAL AND EMPLOYMENT PROGRAMS ON SCHOLASTIC BEHAVIOR AMONG STUDENTS FROM ECONOMICALLY DISADVANTAGED GROUPS

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Introduction

The difficulties of the ethnic and culturally different student within the context of a typical American city ghetto school are quite complex, both in description and solutions. Many compensatory programs have been tried and discarded with little impact on the two primary minority subgroups in city schools, the Black and the Spanish speaking, predominantly Puerto Rican. Where to break the vicious negative cycle of low socioeconomic status, inferior education and housing, has long been a subject of debate within the political, social and educational establishments. But it has been argued that scholastic achievement may be the most logical focus to try to break this desperate cycle.

In a consideration of this problem it was felt that perhaps if students were given a tangible reason to regard school as other than totally irrelevant, for example a paid work arrangement offered by the school in addition to a group experience designed to enhance self esteem and motivation, perhaps there would be more positive attitudes toward school, improved self image and achievement.

Previous Research

The attempt to increase the scholastic achievement of these students has been approached in many different ways. Despite inconclusive research, it is commonly agreed that motivation and positive attitudes toward oneself and school affect scholastic achievement (Sexton, 1961; Coleman, 1966; Banfield, 1968; Silberman, 1970). It has been suggested that inputs other than school, as presently structured, should be considered in an attempt to raise the achievement of low income students. In a nation wide study (Coleman, 1966) it was concluded that certain student attitudes accounted for more variance in the amount of learning than teacher training, physical facilities, or curriculum. The key attitudes were student self concept and the extent to which students believed they could control their future through their own efforts.

Although there is a greater body of published material on cognitive development than motivational development, Passow (1968) felt that the area of motivational research should be explored for its possible potential to raise achievement. In reviewing the literature there is a great deal of information on the relationship of achievement, self concept, motivation and attitudes toward school, but there is a lack of research on the effects of group motivational participation on measured achievement and attitudes toward self and school. Bennett (1955), Roe (1964) and Tyler (1965) listed five common components that should be considered in group motivational work: readiness, consistency, understanding of strengths and weaknesses, information and planning skills, and alternatives and consequences of choice. They also emphasized the importance of self concept in relation to attitudes and motivation.

Work and school have evolved as separate entities in the United States and certain other countries, neither relating to the other, except that level and length of schooling has acted as a credential, or a barrier, for those leaving school and entering the occupational structure (Jencks, 1972). Many students have not been able to take advantage of the educational process and have failed to either complete school or compete successfully while in school. Coleman (1972) pointed out that due to changes in the institutions of family, school and work, young people are shielded from responsibility, held in dependent status, and prevented from participating in productive work, all of which makes their transition to adulthood a difficult and troublesome process.

There is a lack of research on the effects of adolescent work experience programs on achievement and attitudes toward self and school. Passow (1975) emphasized work programs associated with schools as an aid in the development of becoming a responsible person in the transition from adolescence to adulthood. Passow advocated the restructuring and reorganizing of school programs to include work experience as a central component of the education of adolescents.

Considering the societal restraints of ethnic origin, race, and social class, the importance of school achievement as a credential must be stressed to low socioeconomic status youngsters. The school performance of these students is a complex product of many factors. This study was an attempt to determine if a paid work experience for such students (provided by the school), and motivational group participation, have a significant effect on some of the major factors associated with positive school performance, such as achievement, self concept, attitudes toward school, discipline and attendance.

The study will investigate possible alternatives that might be used by schools to stimulate students who view the process of education as futile and irrelevant.

The problem presented for study posed the following key questions:

(1) Would there be a significant difference in the achievement of impoverished students as a result of a paid summer and school year work program and a motivational group course?

(2) Would there be a significant difference in self concept of impoverished students as a result of a paid summer and school year work program and a motivational group course?

(3) Would there be significant difference in the attitudes toward school, including discipline and attendance, of impoverished students as a result of a paid summer and school year work program and a motivational group course?

It is an unfortunate fact that Black and Spanish students of low socioeconomic backgrounds do not generally achieve as well as other more advantaged groups, and this disparity in educational achievement is more pronounced when the nation's inner-cities are considered (Silberman, 1970; Jencks, 1972). While this problem is recognized, at the same time there do not seem to be any major breakthroughs in the more traditional areas of educational research that might help to alleviate the problem of educating disadvantaged youth. By using a paid work program and a motivational group course as an intervention in the school experience of impoverished city youth this study has attempted to provide a model, supported by data, that suggests that achievement could be positively modified while recognizing that it is a complex product of a number of other factors such as student self concept and attitudes toward school, including discipline and attendance (Tuchman, 1973; Werk and Fish, 1973).

Jencks (1972) in a discussion of motivation and work stated: "We doubt that a school can be enjoyable for either adults or children, unless the children feel that they are doing something purposeful. One good way to give children a sense of purpose is to give them activities that contribute to their becoming more like grown ups (p. 257)". The need and importance of work experience as a part of the total school program was presented by Hamburger (1967) in a paper prepared for the U.S. Office of Education. Hamburger stated that such programs facilitate a more relevant school environment, and are especially suitable for students from low income families. Havighurst and Graham (1972) support the opinion expressed by Hamburger that work experience should be offered by the school and that research indicated such programs show promise. Coleman (1975) called for a structured mixing of school and work. He noted schools were viewed as providing an incomplete context for the accomplishment of many important facets of maturation and urged schools to create alternative environments for the transition to adulthood. In addition, a recent U.S. Office of Education survey (1974) pointed out the lack of actual programs and the inadequacies of the few existing ones.

Although there is research reported in the areas of motivation group and work experience programs, it is necessary to determine what type of programs, or combinations of programs, might most effectively be utilized with students of low socioeconomic backgrounds to improve achievement, self concept and attitudes toward school. There is a scarcity of actual studies forcusing on such specific programs. This study was an exploratory one attempting to investigate the specific nature of the relationship between adolescent work, motivational group participation and achievement, self concept, and attitudes toward school of multi-cultural and multi-ethnic low socioeconomic group students in city school systems.

Methodology and Description of the Study

The present study was conducted in a city school district in Nassau County outside New York City. The district serves a school population of approximately 5,000 students and includes children of many ethnic backgrounds with a diverse socioeconomic range. The subjects selected for use in the study were all 14 to 16 years of age. The first step in the selection of the sample was to define a population who met the Neighborhood Youth Corps poverty guidelines. Students were then randomly assigned to one of the four experimental groups so that the variables age, sex, grade, achievement, ethnicity and IQ were randomly distributed throughout the groups.

Group I (N=30) was given summer and school year work assignments only.

Group II (N=30) was assigned to motivational group training only.

Group III (N=30) was assigned to both summer and school year work and motivational group training.

Group IV (N=30) was the control group assigned to neither of the experimental treatments but who received the pre and postests.

Initially, there were 35 students in each group. Due to normal attrition of subjects (always expected in longitudinal studies), the final number in each group at the time of posttesting was 34,32,32 and 33 for groups I to IV respectively. Thirty students from each group were then selected at random so that sizes of groups would be equal. This procedure facilitated the statistical computations and reporting of results for the analyses presented later in this paper. A problem in obtaining appropriate sample sizes was encountered which maybe of interest to others contemplating work in this area. Initially, the goal was to have a sample of 50 students in each group, but there were not enough jobs available during the school year for the 100 students who would have been involved (50 in the work-only group; 50 in the work-motivation combined group).

The pretest and posttest with control group design, one of the most commonly used experimental designs in educational research, was employed and involved random assignment of subjects to experimental and control groups, the administration of posttest to all of the groups. The experimental and control groups were treated as nearly alike as possible except for the treatment variables.

In order to understand the experimental treatments it is necessary to provide a detailed description of both the summer and school year work program and the motivational group participation program.

Summer and School Year Work Program

The Neighborhood Youth Corps was established in January 1965, under the authority of the Economic Opportunity Act. It is administered by the Manpower Administration of the United States Department of Labor and operates throughout the country. The program consists primarily of the in-school, summer and out-of-school work programs and the emphasis is on raising the educational attainment of youth and improving their employability through paid work experience. Program sponsors of the Neighborhood Youth Corps are urged to develop flexible programs to meet the individual needs of their enrollees. Although a variety of services are offered, the primary purpose is to enhance the employability of youth and increase their potential for attaining a planned educational goal.

The stated objectives of the Neighborhood Youth Corps program are to provide gainful employment and other benefits to disadvantaged youth with a high potential for delinquency, increase the potential employability of disadvantaged youth through meaningful work experience and academic training, provide additional income for youth from poor families, provide incentives for enrollees to complete their high school training and to improve their academic achievement and to assist disadvantaged youth to develop responsible attitudes.

The Neighborhood Youth Corps program in Nassau County was begun in 1970 to offer students from low income families job opportunities with local non profit organizations. Such cooperating non profit organizations include YMCA, day care centers, library, orphanage, community co-op center, housing authority, drug rehabilitation clinic, recreation department, N.A.A.C.P., and the city school district. The students qualifying for the Neighborhood Youth Corps program were assigned to one of two experimental groups as described in the selection of the sample. Students were placed in such areas as school clerical or custodial positions, library aides, E.O.C. aides, park and recreation department, city maintenance department, and an orphanage. The rate of pay was $2.10 per hour during the summer and $2.30 per hour during the school year. The students worked 25 hours per week for eight weeks in the summer and 10 hours a week for the entire school year. Each student had a time sheet, which was submitted every two weeks, and was responsible directly to his employer. Any problems or questions were directed to the local work coordinator who was employed by the school district.

Students receiving assistance through the school district's free lunch program were invited to apply for summer and school year work through the Neighborhood Youth Corps and these applications were further screened as to eligibility. It was stressed that there were only jobs for about one of three applicants and that everyone would have an equal chance.

Before actually starting work each student was interviewed by the school-community work program coordinator and by his prospective employer. Two large group sessions covering general questions and concerns, as well as employer expectations, were then conducted by the work program coordinator and follow-up contact was maintained while the students were on the job. It was stressed that school year work assignments depended on a good summer work evaluation. The students in the study actually started work in the summer program the last week in June and worked until the end of the third week in August for a total of eight weeks. Contact was maintained by the program coordinator with the employers on the average of twice a week. It should be noted that the local agencies and employers utilizing Neighborhood Youth Corps students have done so, for the most part, for the three years prior to this study and those employers who have not proven satisfactory were not continued in the program.

After a three week interval students were then employed for the school year beginning in mid-September. They were not necessarily placed in the same position they held during the summer and the work hours were reduced from 25 during the summer to 10 each week during the school year. Students were again evaluated periodically and contact was maintained with their employers. The student participants felt throughout both the summer and school year work programs that they obtained these jobs through their schools and were aware of the contact between the schools and their employers. They expressed little, or no awareness, of government involvement with the Neighborhood Youth Corps and thought entirely in terms of the school as the agent of their employment.

Motivational Group Participation Program

The curriculum of the Motivational Group Participation Program was developed by an inservice committee for the specific purpose of enhancing motivation, particularly of low achievers. The program was used for the first time in this study. Work was begun on the program during the school year 1972-1973 under a grant provided by E.S.E.A. Title I Reading Improvement and Pupil Personnel Services Project, but because of subsequent reorganization of the school system the program was never used except in this experimental study.

The program includes a sequence of 36 units on such topics as increasing one's self worth, goal setting, strengths, values, defining and planning for success and self evaluation. For example, one unit involves "goal reporting" by the students in the motivational groups. The guidelines for the unit read: "Everyone may take turns reporting on his/her achieved goal. If you've been able to achieve your goal in a way that made you feel it was a success, tell how you did it. After you've told about your goal, place a check mark next to your goal chart (designed by each student as part of a previous unit). Then write down one of the strengths you used to achieve your goal. Take a look at all those strengths on your Strength Charts (designed as part of a previous unit). Decide which ones are most important to you . . . After you've decided on your new goal, think about how you can achieve your goal in a way that would make you feel it was a success. Think of a way to use one of your strengths to achieve your goal."

The four group leaders, all counselors in the school system, had a series of meetings to discuss the 36 units in terms of methods of presentation and length of time to spend on each unit. Sixty students from the two experimental groups were randomly assigned to the four group leaders. Each group leader had two groups of about eight students each and each group met twice a week over a period of eight months during the school year. The nature of the motivational group work units was programmatic with each step followed by the four group leaders. The group leaders had a series of meetings prior to instituting the project in September to help insure common agreement as to procedure, uniformity of understanding and clear interpretation of the program. As a result it was not felt that the group leader was a significant variable.

Subjects

The subjects of this investigation were students 14 to 16 years of age from a suburban New York city school district. The school district has a heterogeneous population of whom 20% are considered to be below the county poverty level. This low socioeconomic group is predominantly Black and Hispanic. From the school population identified as low socioeconomic for the purpose of this study, a sample was selected by a simple random sampling method with all students in the defined population having an equal and independent chance for selection. Numbers were assigned to the subjects at random, and the selections were rotated into the four groups until each experimental group contained the same number of subjects. The students were assigned to each of the following groups: summer and school year work; motivational group participation; summer and school year work and motivational group participation. In addition, students were assigned as members of the control group. The random sample procedure was utilized as this type of sample assured the researcher of adequate and equal groups for analysis.

Materials

Three instruments were used in the present study. The California Comprehensive Test of Basic Skills (CTBS) measured the achievement levels of all subjects. The Piers-Harris Children's Self Concept Scale measured self concept. The Students' Attitudes Toward School Survey Questionnaire measured the subjects' attitudes toward school.

Scholastic achievement was measured by the full scale score of the Comprehensive Test of Basic Skills. This achievement test was developed to test skills in the areas of reading, language, arithmetic and study skills. The results of the CTBS have value as a survey of individual and group performance in basic skills, and as an analysis of learning. The tests can be administered and used by classroom teachers having a minimum of formal training in standardized testing.

The Piers-Harris Children's Self Concept Scale was used to measure self concept and was designed for use as a unidimensional measure of self concept. This scale is appropriate for grades 3-12 and consists of 80 first person, self judgment, declarative statements of children's feelings about themselves. The scale was originally standardized on a population of 1,183 students covering a wide age range. The use of this scale for research was emphasized and it was recommended for studies of change in self concept. It was recommended that a control group be used because the scores on the scale tend to increase slightly with retesting (Bentley, 1972.).

The Student Attitudes Toward School Survey Questionnaire was utilized to determine attitudes toward school. The reliability and validity of the semantic differential technique, the measurement technique used in the questionnaire, has been established by Osgood and Snider (1969), and Osgood, Suci and Tannebaum (1957). Retest reliabilities yielded coefficients in .80 to .90 range, and the validity of this type of scale appears high (Brinton, 1961; Solarz, 1963).

Statistical Methods and Experimental Design

The following steps were taken in the analysis and treatment of the data. Six pretest measures were obtained on the 120 subjects: grades, CTBS scores, Attitudes Toward School Survey Qestionnaire, school attendance ratings and school discipline referrals, and self concept scores. Scores on the Comprehensive Test of Basic Skills (CTBS) were reported as total raw scores. Final grades were reported as an average of all subjects where an F equalled 0 and an A+ equalled 12. The Piers-Harris Children's Self Concept Scale was based on the raw score of the number of positive responses as was the Attitudes Toward School Survey Questionnaire whose 20-item, 7-point scales in each of the three concepts yielded a possible range of scores from 20-140. The scores for the three concepts were averaged to one total score.

Subjects were then assigned randomly to one of the four treatment groups: the work experience condition, group motivation participation, combined work experience and group motivation participation, and a control group. Pretests were given at the end of the school year (May-June, 1975) prior to the year during which the experimental programs took place (1975-76). Every subject received the six pretest measures and ratings and was assessed on the same measures and ratings one year later (May-June, 1976; posttest results are reported in Tables 1 and 2). To test the hypotheses of this study a single classification analysis of variance was done on the six dependent variable measures at pretest and at posttest following the training procedures. The .05 level of confidence was accepted for significant differences. The four treatment variables; the work experience group, group motivation participants; the combined work and motivation group and the control group, made up the classificatory groups for each of the twelve ANOVAs. The CTBS, grades, the Piers-Harris scores, attitudes toward school ratings, attendance figures and discipline referrals were analyzed in the cells of these twelve ANVOAs (six dependent variables, pretest and posttest).

The adequacy of the procedures for randomly assigning subjects to the four treatment conditions was verified by analyzing the differences between the treatment groups on pretest. The hypotheses of the study predict significant differences on the posttests. Where significant F ratios were found, the Tukey t test was used to determine where the significance existed internally. Justification for use of the Tukey t is provided by Winer (1962) and Hayes (1963) as they noted it was preferable when the various samples were of equal size and the experimenter was interested in all possible pairwise differences between means. The .05 level of confidence was set to determine whether there were significant differences.

Results

This study hypothesized that exposure to a summer and school year work program, a motivational group participation course and a combination of the two programs would result in increased achievement, more positive self concept and attitudes toward school, fewer discipline referrals and better school attendance. Results showed the following (significance refers to the .05 level).

1. No significant differences were found for the four experimental groups on achievement, self concept, attitudes toward school, discipline referrals and attendance at pretest. This was predicted because of the procedure adopted to assign subjects to groups at random. The rest of the results reported immediately below refer to analysis of the posttest scores, which are presented in Table 1.

2. In regard to achievement, it was found that none of the experimental group treatments yielded significant differences when measured by a standardized test, the Comprehensive Test of Basic Skills. When achievement was measured by grades, however, the analysis of variance indicated significance. When the mean grade scores of the four groups were compared with each other the work group (Group I) and the combined work and motivation participation group (Group 3) had higher mean grade scores than the motivation participation group only (Group 2) and the control group (Group 4).

3. When the four groups were compared on self concept the results indicated no significant improvement for any of the groups after the three experimental treatments: work experience, motivation group participation and the combined condition. However, there was a trend toward higher mean scores on self concept for students who participated in the motivational groups (Groups 2 and 3).

4. In a comparison of the four groups on student attitudes toward school it was found that there was significant change on measured attitudes toward school. The combination of the work experience and the motivation group (Group 3) had higher mean scores on the Attitudes Toward School Survey Questionnaire than the work-only group (Group 1), the motivation participation group (Group 2) , or the controls (Group 4). The work-only group had a higher mean score than the controls.

5. In a comparison of discipline referrals no significance was found for the treatments on the four experimental groups. Reported mean scores indicated improvement for the 2 groups which received work experience but not at the .05 level which was adopted in this study (The actual level was p. 10).

TABLE 1

Results on Posttests, and Ranges of Scores, for Achievement, Self-Concept, and Components of Attitudes Toward School

		Achievement		Self	Attitudes	Discipline	Attendance
		Grades	CTBS	Concept	Toward School	Referrals	(days absent)
Group 1 (N=30)	Mean	5.46	6.72	54.8	97.0	4.3	7.0
Work Group Only	S.D.	1.97	1.58	8.8	10.5	8.3	3.3
Group 2 (N=30)	Mean	4.10	6.80	56.4	94.5	8.3	10.0
Motivation Group Only	S.D.	2.30	1.46	7.1	7.1	9.5	8.9
Group 3 (N=30)	Mean	5.40	6.75	54.7	99.0	3.8	5.6
Combined Work and Motivation Group	S.D.	1.84	1.50	7.8	5.9	5.4	2.0
Group 4(N=30)	Mean	4.60	7.17	51.6	94.4	6.8	6.9
Controls	S.D.	2.11	1.74	7.2	7.1	11.7	4.5

Range of scores obtained

Achievement: Grades	0(F) - 10 (A minus)
Achievement: CTBS	3.2 - 11.5
Self-Concept	33 - 72
Attitudes Towards School	77 - 115
Discipline Referrals	0 - 57
Attendance (days absent)	0 - 46

6. In comparison of the four groups on student attendance it was found that there was significant change on reported attendance patterns for the groups after receiving the experimental treatments. The combination of the work experience and the motivation group (Group 3) had better attendance than the work-only group (Group 1), the motivation participation group (Group 2) or the controls (Group 4). Results for the work-only group, compared to others who did not receive work experience, approached significance but narrowly missed the needed .05 level of confidence. The findings are summarized in two tables which give posttest scores for all variables (Table 1) and the results of the statistical analyses (Table 2).

TABLE 2

Results of Experimental Treatment Groups on Achievement, Self Concept, and Components of Attitudes Toward School

	Achievement		Self Concept	Attitudes Toward School	Discipline Criterion	School Attendance
	Grades	CTBS				
Group 1 (N=30) Work Group Only	.05	N.S.	N.S.	.05	Pos. Trend	Pos. Trend
Group 2 (N=30) Motivation Group Only	N.S.	N.S.	Pos. Trend	N.S.	N.S.	N.S.
Group 3 (N=30) Combined Work and Motivation Group	.05	N.S.	Pos. Trend	.05	Pos. Trend	.05
Group 4 (N=30) Controls	N.S.	N.S.	N.S.	N.S.	N.S.	N.S.

Entries refer to levels of significance. Pos. Trend means a positive trend toward significance (.10 >p. >.05).

Effects of Experimental Treatments on Achievement

The experimental groups that received the summer and school year paid work experience, the motivation group participation course and the combination of the two treatments did not demonstrate significant change when measured by the standardized test, the Comprehensive Test of Basic Skills. Perhaps if more time were devoted to preparation for taking standardized tests, such as going over the format and taking practice tests, thus not being intimidated by them, then better results might be achieved when measured by an instrument such as the CTBS. However, significant results were attained when the two experimental groups that had work experience were measured by achieved grades. It would appear that performance on the structured standardized test cannot be altered by the types of treatment in this study. Self concept, attitudes toward school and achievement are interrelated (Brachman, 1970; Frerichs, 1970; Felker, 1974) and grades are influenced by such factors as effort, behavior, attendance and general attitudes toward school. Thus, it is understandable that achievement as measured by a standardized test did not show improvement, but when measured by teacher grades the results were significant.

The results of this aspect of the investigation supported the findings of Read (1968) and Altschuler, Tabor and McIntyre (1970) when it was reported that an experimental group exposed only to the motivational group program did not demonstrate significant change in achievement, either on the standardized test (CTBS) or teacher grades. The motivation group participation experience was not in itself enough to affect achievement.

Effects of Experimental Treatments on Self Concept

An analysis of the data on the effects of the experimental treatments on self concept revealed that the results were not statistically significant. A review of the literature revealed that Parks (1968), Messana (1969), Aspy (1971), Altschuler and Irons (1973) and others have found that various types of work programs on group participation to enhance self esteem did affect self concept.

It can be speculated that perhaps the work experience did not result in more positive self image because the work eligibility guidelines necessitated selections from low income families comprised predominantly of ethnic minorities. So while the students wanted the work it did not improve their image of themselves. This has been identified and discussed in the literature by Greenberg (1970) and Paton (1973).

However, results from the motivation group (Group 2) approached significance. Evidently the participants in the motivation program felt better about themselves, as a positive trend was indicated, but not at the required level of significance. It should be noted that the lessons of the motivation group specifically focused on self concept. The positive trend is encouraging as this was the only area where favorable results were reported for the motivation group alone, not in combination with the work group in the study. The students seemed to welcome the opportunity to express themselves and evidently improved their self image, but there was no transference to any of the other variables. It would be interesting to determine if this more positive self image was maintained, and if variation of the motivational group participation format could possibly lead to significant results, not only for self image but in other areas such as achievement as well.

Effects of Experimental Treatments on Attitudes Toward School

An analysis of the data for the effects of the experimental treatments on students' measured attitudes toward school revealed statistically significant results. The greatest significant mean score gains were found for the combined work and motivation group (Group 3) in comparison to the other three groups. The results of this investigation supported the findings of Murray (1962), Green and Stachnik (1969), Schneider (1971), and Wynne (1971) who reported work experience had a significant positive effect on students' attitudes toward school.

It should be noted that the low socioeconomic sample used in this study expressed more negative attitudes toward school than the norm and reported scores in the lower ranges at pretest on the semantic differential. The measurement of the attitudes of the sample used in this study could show improvement yet still score in the normal range for an average school sample. The jobs were a prized addition to their lives and for most of them this was the first time they had worked on a formal, weekly basis They regarded the school as the source of their employment and felt that the school had really done something for them. They also felt that if the job was given to them it could also be taken away. This impression was not conveyed to the students but it was a safe assumption that many of them did not want to jeopardize their income, and as a result behaved better in school, which in turn affected their grades, attendance and number of discipline referrals. Paton (1973) noted that low socioeconomic status was correlated with a failure syndrome in innner-city junior and senior high school students. One way to enable the student to have more control over his environment is to provide paid work which can result in a different perspective and meaning to his schooling, especially if he perceives the school as the means of employment (Paton, 1973).

Effects of Experimental Treatments on Discipline Referrals

No significance was found for the effects of the experimental treatments on the number of discipline referrals for the four groups. Although the results of the analysis of variance approached significance, the .05 level was not reached. Thus there was only a trend toward less discipline referrals for the experimental groups. The review of literature found no studies that evaluated the issue of work and motivation group experience solely on a discipline criterion. However, it was generally agreed that discipline was an aspect of general attitudes toward school (Coleman, 1969). The lack of significant change in the number of discipline referrals for the experimental groups as a result of the treatments might be at least partially attributed to the fact that even though the work-only group (Group 1) and the combined work and motivation group (Group 3) did improve in comparison to the motivation-only group (Group 2) and the controls (Group 4), the low income students in this study had a history of higher than average incidence of difficulty in school. This type of reputation is not easily overcome in just one year. It would be interesting to determine if the trend noted here would reach statistical and practical significance over a longer period of time.

Effects of Experimental Treatments on School Attendance

Significant differences were reported for the effects of the experimental treatments on school attendance for the four groups. Attendance was measured by comparing the total number of days absent on the official district attendance records at both pretreatment (the year prior to this study) and after treatment. It was found that those students who worked during the summer and school year (Group 1) had much better school attendance. This was not found for the motivation group (Group 2). It was also found for the combined treatment (Group 3) as that group included the work experience as well as motivational group participation.

As previously noted in the comparison of attitudes toward school and work it was felt that the positive effects of the paid work experience enabled impoverished students to regard school in a different manner. This more positive attitude toward school affected not only their grades but their attendance and number of discipline referrals as well. The students may have felt they had a reason to come to school, or perhaps something to lose if they did not attend. The results of this study indicate the possibility that school related factors such as grades, attitudes toward school, and attendance can be significantly improved for low income students by such attempts at intervention by the school as paid work experience (Wynne, 1971; Havighurst and Graham, 1972; Levine, 1974).

Concluding Remarks

The study was limited by the size of the student sample and the location of the population, as it was representative of one suburban New York city. The results are descriptive of this sample and any application to other populations must be done with caution. With respect to instrumentation the study was limited in its use of the California Comprehensive Test of Basic Skills and final grades to measure achievement; the Piers-Harris Children's Scale of Self Concept to measure self concept and the Attitudes Toward School Survey Questionnaire to measure attitudes toward school. Generalizations from the study were also limited by the specific type of group motivation program and the type of paid work experience project used in this investigation.

Although the inferences made in this study pertained only to the sampled group and despite the limitations noted above, the findings should have implications for all those concerned with the attempt to reach students with impoverished backgrounds. The findings of this study resulted in the formulation of the following recommendations.

1. Research should be conducted to examine the effects of different types of work experience programs, not just the Neighborhood Youth Corps. School systems should consider the development of their own work programs with the utilization of local community resources.

2. A longitudinal study should be done to determine whether the positive effects noted last for longer than a school year. One study may measure these effects after termination of a similarly constructed work program while another may measure the effects of a work program lasting for more than one school year.

3. The motivation group program should continue to be developed and further evaluated so as to fully explore its potential.

4. Further research is needed to explore the possibilities of different types of motivational courses for school settings. Such research might include an analysis of the various responses of different age groups and longitudinal follow up to determine the duration of results.

5. One of the indirect results of this study was the reinforced realization that low socioeconomic group younsters do not score well on standardized tests. Studies are needed to focus on specific efforts to help such students become more familiar with standardized tests and test taking techniques.

6. The components of the present study could be used in a comparison study of advantaged and disadvantaged students to determine if they differ significantly in their response to the treatment groups.

7. The results described in this study are limited to students over 14 years old. Research should be conducted to determine if a similar study with younger students would yield significant results.

In summary, this study evaluated the effects of a paid summer and school year work program, a program of motivational group participation and a combination of the two programs, on the achievement, self concept, attitudes toward school, school attendance and school discipline for a sample of multi-ethnic and cultural low income students. The purpose of the study was to measure and compare the effects of the experimental treatments on the variables after an interval of one full school year. It was found that work experience did significantly affect achievement (as measured by grades but not by standardized tests) and attitudes toward school and attendance. There was a positive trend ($p. < .10$) noted on the criterion of fewer disciplinary referrals. The motivation-only groups had no affects on any of the dependent variables but a trend was evident for self concept.

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IN A JERUSALEM ULPAN

CYNTHIA PINCUS *

This paper will analyze some of the many phenomena within an intensive Hebrew ulpan[1] in Jerusalem, which is presented to students as simply a language course, but is in fact a social system facilitating the entry of immigrants into a vastly different culture.

In every country with major waves of immigration, the process of acculturating adults has been a topic of interest to educators and sociologists alike. The international literature describes a variety of techniques and philosophies behind the formulation of courses for immigrants in history, citizenship, and language. Equally as important as the technique of teaching language is the environment in which it is learned. As Shaked has pointed out,[2] there are a number of simultaneous goals, including 'co-operation in the creation of cultural values, providing a spiritual leader, satisfying student needs—including surrounding them with social warmth at a time when many are bereft—and providing discipline in study.' In addition, a small, intensive group ulpan takes on many of the features of a 'transition group,' in which the teacher—whether conscious of it or not—serves as the group leader and representative of the new culture simultaneously.[3]

Israel is a new nation, born out of immigration, and dependent upon immigration. Following statehood, one of the nation's first acts was to promulgate 'The Law of Return,' meaning that any Jew, from anywhere in the world, could come to Israel. This open-door policy has paved the way for an unprecedented flood of rapid immigration without selection, encompassing the skilled and unskilled, the healthy and the sick, and even the dying. Immigration is Israel's lifeblood, the source of her strength. It is also the source of a variety of social problems inherited from all over the world: the Atlas mountains, the mellah of the Moslem cultures,[4] the concentration camps of Europe. From 1948 to 1970 1,399,112 immigrants entered the country, with the highest proportions coming from vastly different cultures: 36% Eastern Europe, 28% Africa and 23% Asia.[5]

While immigration is sought by Israel actively, there is another rather unique aspect to it: the high value placed upon the act by the Jewish people for over 2000 years. In the Bible we find such admonitions as

> 'Get thee out of thy country and from thy kindred, and from thy father's house, unto a land that I will show thee!"[6]

Immigration to Israel is an act which is highly valued not only in the Israeli culture, but

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throughout diaspora Jewry. At the annual Passover Seder, Jews all over the world exclaim 'next year in Jerusalem!' In Hebrew, going to Israel is 'aliyah' or 'going up' while leaving Israel is 'yaredet' or 'to descend.' The importance of attracting immigrants is accepted throughout Israeli society, and, consistently, special efforts are made to attract Israelis abroad back to the country.

Similarly, to be studying Hebrew is considered an important activity, with status and value connotations unknown in other established societies. In Israel, almost everyone has struggled with the language personally,[7] and people are highly supportive of newcomers' efforts to learn. This can be carried quite far, as when for example the author has seen shopkeepers ask customers please *not* to help a person in his native language as he is trying to learn to order in Hebrew. Similarly, inaccuracies of accent and grammar are tolerated to an unusual extent. This widespread social support for learning Hebrew is further underscored by the prevalence of teaching aids throughout the culture: easy Hebrew newspapers and radio news for immigrants, and a television ulpan[8] in prime-time—although Israel has only two channels, and competition for time is great.

Ulpanim are found by the dozens in Israel: on kibbutzim, at universities, in community centers. In the summer there are ulpanim for all school children from kindergarten up who will be entering Israeli schools in the fall, and these classes continue into the winter for those not yet ready to attend an all-Hebrew school. At the universities, there are graded Hebrew classes for new students, faculty, and visiting professors. In order to reach all kinds of students, there have been ulpanim in settlement camps, in moshavim,[9] in the morning for housewives, at night for workers. For traditional women who have known no role outside of the domestic, there have even been Hebrew lessons slipped into cooking instruction,[10] or located at laundromats to transform language study into an 'acceptable' occupation.

The ulpan to be discussed here is one of the most intensive available. The 30-hour-a-week schedule is a demanding one, but enables students with no background whatsoever to read, write and speak Hebrew in three months. Intensive ulpanim are in great demand and new classes start continuously. Tourists are generally not attracted to such programs; those who commit this amount of time have come to Israel to work, study, or stay. Because the class meets 6 out of 7 days a week from 8 a.m. to 1, it becomes the central focus of all the participants, with both the increased motivation and frustration inherent in the student role. This schedule also creates a very intensive group experience over the three month period.

Characteristics of the Group

Groups are kept at about thirty students, of any age over 18. While student-age men and women are eligible, relatively few are included simply because students in the educational systems receive their Hebrew instruction in them. Thus these classes are predominantly made up of adults not accustomed to the student role, and a few student-age individuals who are out 'of the system' for various reasons, e.g. highschool-aged drop-outs, college-aged students who are not college material, or college students taking a year off. In the class to be discussed here there were a majority of older people, and a majority of Russian and Eastern Europeans. This is now most often the case given recent patterns of immigration, with 4,000,000 Jews immigrating to Israel in the '70's from the Soviet Union, and an estimated 35,000 more Russians in the past year.[11] The composition of the group included 11 Russians, 4 Rumanian, 1 Canadian and 4 American immigrants. There were also 3 Polish refugees from Warsaw, 2 having lived in Denmark for some years, the third fleeing to South America and living there and in the United States for most of her adult years before emigrating to Israel. Another small group consisted of those who had not immigrated, but were working or studying in Israel for a year or more from a variety of personal motivations: 1 Swiss, 4 Americans, 1 English, 1 Finnish.

The group was as heterogeneous as a group could be. It combined not only nationalities, but those from free and unfree societies, Jew and Gentile, permanent and temporary, religiously observant and non-observant, men and women, and youth (from 19) to age 78. It was immediately clear that there were still other major differences and that we had both the firm and infirm; one student had a severe hunchback, another a bad back which forced him to stand much of the lesson. An old man had a limp, needed a cane to walk with great difficulty and could not see the blackboard or his notebook because of a cataract operation. Clothing indicated similar extremes of wealth and poverty; the Westerners were immediately identifiable because of their new, bright clothing and shoes, belts, jewelry and purses, while the Russians had simple and faded clothing, few accessories, and a proliferation of gold teeth in the front of their mouths which were in evidence much of the time as they smiled easily and often.

The classroom immediately provided a 'melting pot' in the extreme. Here everyone was simply a student. This meant that everyone was also stripped of status and identity by the inability to explain himself to fellow students with any additional information. The professor and the laborer met as equals. Family, class and accomplishments were irrelevant—and this was most uncomfortable for those who had the most to lose; the prestigious, the doctors, scientists and professionals. The teacher also introduced herself simply by using her first name, and she clearly avoided supplying any of the lost pieces of identity. This lack of interest and emphasis on what you were last year or last week carried with it a major dimension of culture shock. (As a psychologist put it to me at the end of her first week in Israel, 'You know, I have no idea suddenly of who or what I am'.) But this shock must be dealt with because in the new culture each individual will have to find a new definition of himself as an Israeli, and the sooner the process begins, the faster the adaptation.

After learning to say "student" and "teacher" we are immediately taught the term "ole hadash" or new immigrant, and this enables us to identify ourselves as such. "Ole hadash" is a term one hears day-in and day-out in Israel. People brightly ask us if we are; in taxis and busses and shops, and it always hurts to say "no" and see their faces fall. Most often, if we say "no," the response will be "ah, but maybe, next time," or "if you learn Hebrew you can so easily stay on..." To be an "ole hadash" is to have made a commitment and cast one's lot with the people. The seven of us in the class not in the process of immigration felt badly about our different status, a little like guests who are invited to a party because they happen to be around.

This first morning we introduce ourselves, having just been taught terms for "my name is" and "I am from." This is the sum total of equipment provided.

The Group becomes a Group

At the start, this was a group of 30 individuals bonded together by a common purpose, but a unique group in both its heterogeneity and its lack of a language. Just as in a blind group, there will be heavy reliance on touching, in a group without a language to share, there is heavy reliance on the non-verbal, and on eye contact with much smiling, nodding, laughing, and exchanging friendly glances. There is also an immediate and strong investment in the task of the group,[12] learning Hebrew, and students assume the role of 'good student', with great interest in the teacher and the lesson.

As the group was now to be together every day for almost five hours, with two 'hafsakas,' or recesses, during the morning, the social aspects of the group were intense. Curiosity became a major motivating force. All their lives, the adults had used language as a tool for getting to know people. Now each was confronted by a variety of people whom he would like to know and understand, but he was virtually tongue-tied.

The teacher did not indicate any guidelines for the social breaks after showing the students where the cafeteria was, and if in the cafeteria at all, sat at a separate table of teachers. The non-verbal communication which started in the classroom was relied on heavily at this point. The Russians shared a bag of nuts, people admired a woman's dress, etc., all mostly in pantomine. Two or three class members were able to construct a rudimentary sentence in Hebrew with which to communicate but this proved highly frustrating. It became clear that there were several languages shared by quite substantial subgroups. The American and Canadian students could converse with those from Denmark and Finland in English, the Russians could converse in Russian with the Eastern Europeans, and everyone over sixty, whether from Latvia, Brooklyn or Warsaw, could speak Yiddish, although no one under that age, from any country, could speak it at all. There were two additional languages in the group, French and Spanish, which went unused. These language subgroups introduced other possibilities. In addition to making a group accessible if you knew the language of entrance, you could also use another person who had two languages to translate your way into an additional subgroup. There were pressures on the students to remain with those of the same language group, not only because they sought one out, but also because of the real difficulties of living a social life at kindergarten level. If one used a translator to bridge the language gap, there was always the risk of his leaving—and then one was alone again at a table of students with whom one could not communicate at all—an anxiety-arousing experience.

As an intensely interested professional observer, I performed an experiment in group process (while ostensibly in the role of student) to see if I could expedite the crossing of these language boundaries. From the first hafsaka, I floated through all of the different subgroups, either communicating non-verbally, or through translators. Almost all the students welcomed this aggressive friendship from an outsider—which was completely sincere—and were highly responsive to it, taking extra efforts to include me in their interaction.[13] While this cross-lingual activity seemed to come most comfortably to several of us who had had professional experience, it was gradually emulated by all of the more sociable members of the class so that 50% of the class was mingling with other language subgroups after the first two weeks had passed.

I further attempted to increase communication (and avoid being 'frozen' into my own language subgroup) by changing my seat every day in the classroom. The first three days several individuals protested 'that is my seat' when they arrived later and found me where they had been the previous day, but as my seat rotated, everyone else's did also, so that all students became acquainted with a variety of other students by sitting next to them. While the teacher had never directed us as to seating, she responded to my changing my seat and the students' initial protests, by saying 'There are no fixed seats,' thus lending approval to our mixing. The response to my initiatives showed how easily the students' feeling of group-ness could be strengthened and accelerated in such groups, by leaders, by teachers, or by explicit directions from the teacher as part of the norm-setting process.

The teacher as culture-bearer

Our teacher accompanies us to the cafeteria on the first day only, and just to show us where it is. She has pale, pink skin, and blond, almost white, hair. One woman asks her in broken English, "Are you Ashkenazi?" She laughs, "I do not know what you mean." "But in America we ask this question often." "What is Ashkenasi?" the teacher persists. "Eastern-Europe. Sephardic—Spain, Morocco. *I* am Ashkenasi." The teacher shakes her head. "In Israel we do not say such a thing. We are all Israeli." She pauses, thinking, then adds, "One might say where are you from, but never are you 'this' or 'that'—that is not nice. It means nothing here. Actually, my parents are from Hungary." At that moment it first becomes clear that she is to be *guide* as much as teacher. That language will be taught in context, and that that context will be the whole of Israeli culture. And this first lesson is an important one. We are to catch on, gradually, to the fact that Israel is struggling to dispel the great social inequalities of the different currents of Jewry which have poured into the country from all over the world. Fifty-one and seven-tenths percent of her immigrants have come from African and Asian backgrounds, culturally deprived in striking contrast to their Western counterparts from Vienna, Berlin and New York . . . Meanwhile, the connotations of origin among Israeli Jews can carry far more stigma than elsewhere in the world. This goes against the ideal, and is fought, strenuously.

The leader sets the norms for the group

As those familiar with small group processses know, standards, or norms of behavior tend to be established quite quickly at the beginning, and help the group proceed towards its goals by giving its members a feeling of belonging. In the ulpan, the European educational tone prevailed, with little time or attention being given to explanations for the way things are done, or expression of feelings about them. The teacher set norms for the class firmly, simply, and from the first hour, introducing them as each occasion arose. The class was to start immediately, both in the morning and after each break, and continued exactly to dismissal time. The teacher taught in a brisk, infectious rhythm with no explanations in any other languages; not only would it be impossible to stop for explanations in 6 different languages, but it was also clear that no explanations were needed. The system was built on the introduction of words in context, with repetition by the students until the pronunciation was mastered. The teacher is the absolute authority, although half the age of many of the students, and sets the pace and tone. If students were late or absent, they were questioned in front of the class, while those with incomplete homeword were scolded—all reinforcing the idea that this was a serious undertaking.

> Teacher: Do you want to learn Hebrew?
> Student in her sixties: Yes.
> Teacher: How will you learn Hebrew if you don't study at home?

A habitual latecomer is questioned again and again:

> Teacher: Why are you so late—it's 8:45?
> Student: I was sick.
> Teacher: Everyday you are sick? Everyday you are late.

The parental firmness and absolute expectations were extremely effective. While foreign language courses for adults can meet with lethargy, and a general lack of commitment, this course was clearly 'top priority,' and this was grasped immediately by the class. Attendance was taken, formally, each morning. Assignments must be handed in and they were returned, carefully corrected. Eating and smoking in the class were forbidden, as was the use of any language other than Hebrew in the classroom. The importance of completing the course, and getting a diploma—again the symbol of its importance—was stressed again and again.

The value of these norms became obvious. Most of the students had not studied for years and many were shaky in their motivation to begin with. The determination of the teacher provided the students with an important example, and they quickly adopted her norms as their own. The teacher was pleasant, but maintained her distance by not associating with the students during breaks, nor getting to know individual students well. While she offered a great deal of support, it remained collective, rather than individual.

The Goals

Adopting the teacher's norms as their own, the students worked hard at mastering Hebrew. Hebrew is vastly different from languages of the Western world. First come the ABCs; aleph, bet and gimmel. This is accompanied by the explanation of a number of idiosyncracies of the language. While the other languages we have learned run left to right, Hebrew runs right to left, although numbers continue to be written left to right. Odd-looking letters are introduced and the students practice writing them over and over again—the hand must be trained to move 'backwards' and the letters cannot be connected as in Western script. Our notebooks, like all Hebrew books, must begin at what we know as the end. We learn the alphabet in both its written and printed forms, stumbling over the sounds like first graders. Because Hebrew is a democratic language, having no capitals, our efforts to read are complicated by the absence of any clues for the introduction of a common name, so that words like 'Portugal' and 'Argentina' can confuse us for long periods of time. To further simplify, vowels are omitted—not only in class, but on signs, in letters and newspapers—and for beginners, the result is a puzzle without all the pieces. As modern Hebrew has been streamlined for daily use by several million people, many common words are abbreviated now to tremendous extents, so that Mrs., 'gveret' appears roughly as 'gv' and afternoon, 'hasohorium,' simply as 'tsh.' These symbols must also be mastered.

Traditionally, in Hebrew, letters have numerical value, and students must learn these equivalents too. The days of the week, the grades of a school, and the dates of the Hebrew calendar are all lettered—we are even taught to do mathematical sums with letters. We are prepared for notes that will come in the mail referring to events on day H or G, or a meeting in room C class D. Additional orientations were needed, for many students, about weights in kilograms, distances in kilometres, the months of the Hebrew calendar, all basic to the structure of Israeli life.

Unlike students learning a foreign language in their native lands, immigrants need far more than just the language of their new country. They need a feel for their new world, a basic orientation. Culture shock is not only the sudden cutting off from the written and spoken word—although this isolation greatly increases its effects. It is also what has been called cognitive disorientation,[14] the loss of the familiar signposts and references, followed by a gradual tuning in to new smells, temperatures, foods and routines.

It is not enough to educate the immigrant to speak. He must also be prepared to become part of the culture itself, to participate in its society, goals and political process. Immigrants need immediate orientation to the Israel culture: part Judaism, part European, part Middle-Eastern. Orientation requires quick access to a great deal of information about what is important, what is accepted, how to cope, and how to live a good life under markedly different circumstances.

One way of tuning in is learning idioms in context. While idioms are important in any national acculturation, they seem even more so in Israel, where modern Hebrew is still a language in the process of formation, and a small vocabulary is overworked—people use the same words and expressions over and over again in the same situations. This restriction of vocabulary is exaggerated further by the high percentage of immigrants among the adult population. For those weak in Hebrew because it is their second, third or fourth language, synonyms and subtleties are out of the question.

We were taught useful idioms right away. 'Lama? Ka ha.' 'Why?' 'Because.' It sounded like a joke, but we had to learn that this was the usual response when one questioned or objected to a prescribed routine in an office or institutions.

> why is the office closed when it should be open. ka ha.
> why can't I just go inside to speak to that man? ka ha.

'Shalom,' literally, peace, is used for both hello and goodbye and sometimes even welcome. Yet its obvious significance in a country longing for peace for a change is at all times clear. 'Shalom' says a housewife to an ice cream vendor. 'I hope so,' he replies. 'Shalom to all Israel,' says a kindergarten teacher, dismissing her little class.

To ears attuned to Yiddish, familiar words spring up everywhere, in unexpected places. While bagels are non-existent in Israel, round rolls are termed "bagelas." A mental hospital is a "Bet mishagas." Equally important are the hundreds of words not present in modern Hebrew and therefore used exactly as in English only with new, Hebrew pronunciations; universita, museum, concert, frigidair, nescafe, studente, hallo . . .

Deciphering the language was only part of the learning process. Immediately, the students embarked on the process of learning the cultural orientation necessary for adaptation. This started with a thorough explanation of the Jewish holidays, which are occasions in Israel where everything stops: schools, banks, stores, movies and even transportation. The holidays are national, and as cultural as they are religious. The students were taught exactly what they would find people doing on these days, so that they could consider their own roles in relation to it. For example, on Rosh Hashana, cards for the New Year are sold on tables on the streets, and we were taught precisely what we could say, or write in the way of appropriate greetings. The teacher went over the various items one might find on Israeli tables: honey for a sweet year, the head of a sheep for the head of the year. While the presentation was cultural rather than religious, many of the comments revealed that for the Russians, although Jewish, much of Jewish culture was as new as for the Gentiles in the class. Several Russian students had no idea of the best-known Old Testament stories, and were incredulous to learn that in the United States well-accepted intellectuals and professionals send their children to Jewish day schools where they are taught both religion and Hebrew.

The news broadcasts in Israel come frequently throughout the day and most Israelis switch them on often—at least to catch the headlines to be sure 'all is well with us'. News is highly national in focus, concentrating on Israeli events, security matters, and the fate of Jews in the diaspora. The teacher elaborated on items in the news of particular significance during the first hour of the day, and the students had the opportunity to discuss these concepts—many of them startling and new—as they learned the terminology for them. After one of the daily terrorist incidents, the teacher explained

> Once at the university, an Arab student asked me if I knew how to swim. I said 'Yes'. 'That's good' he said, because we are going to drive you into the sea'. She added, very quietly, 'When there are wars you see, we have to win—there is no where else for us to go.'

Terrorists and war are daily topics in the press, and so they became daily topics in the classroom. Students had to incorporate these risks into their awareness, until they reached the point where they stopped being obsessed, and took on the Israeli stance of 'life as usual in the face of threats.' Working this through emotionally, the class could turn to other topics in the news, important to the nation and to Jewry. An example was the Jackson amendment—everyone had a strong opinion on its effects:

> 'Not more than one and a half million will come,' a Russian says, 'How many Jews in Russia still even call themselves Jews? Sometimes you can tell by the name—perhaps there are really not three but five million of us in Russia . . .' An elderly Russian woman is shocked; '*all* Russian Jews will leave! but the problem is where will we put them here?' 'That will not be a problem,' the teacher comments. 'After 1948, our population went from 600,000 to 3,000,000 in 5 years. Every family with an extra room had an immigrant family in that room. No beds, no money, no place of their own—they slept on the floor. It made no difference, they were here. 60,000 will be much easier. If they want to come, they'll come.'

Once the autumn series of holidays was over, the teacher introduced a variety of techniques, all for the purpose of teaching basic language, but embodying the social skills which adults would need to cope. 'Sihas,' or conversations were memorized: little slices of life—friend greeting friend, arranging to meet for a movie, looking for an apartment—essential, basic phrases committed to memory. Memorizing, however difficult, had the effect of enabling students to come out with the correct phrase in the correct situation spontaneously, even long before they could read, write or express themselves in the language.

We were taught to write a letter, word a telegram, read and pay bills. The bill-paying was especially urgent, in a country where, if one does not pay a phone bill for example, the phone is cut off and a heavy fine exacted—within 48 hours of the delinquency.

As the class became more spontaneous, students were asked to role-play as they used the phrases which had been taught. They embellished with pantomime and humor. This role playing not only increased social skills but also accomplished some additional things. The class became cohesive as the students got to know one another more deeply. Led by the freest and most imaginative 'actors,' all the students learned to laugh at themselves and their predicament. The teacher encouraged this and invited the students to trade stories about their worst mistakes in the new language:

> I ordered mahar (tomorrow) in the restaurant instead of marak (soup) and the waiter thought I was crazy . . . I was buying a toy for my son and I asked for neft (gasoline) 'which makes a loud bang' instead of nesek (gun). My husband said they would have me arrested for a terrorist.

The role-playing continued; we tried asking directions of each other, ordering and serving restaurant meals, and even bargaining.

As the acculturation through the language progressed, the teacher explored the students' adjustment.

> Do you speak Hebrew, Abraham? Yes, on the bus.
> Street Hebrew.
> And do you speak Hebrew, Ira?
> No, I do not.
> What do you speak on the bus?
> Nothing, I don't speak.
> That's not good—you must try—you are speaking Hebrew now!
> Sarah, are you speaking with your neighbors in Hebrew?
> No longer.
> Why?
> Because they are always asking me why I came here. It upsets me. They do not want us.
> But I think you misunderstood.
> They really want to know.
> Why are they here? Because somebody else came. You may be the first recent immigrant they have met and they may not understand this.
> I don't know.
> Try. Slowly, slowly, talk a bit. A bit at a time . . .

This theme was continuous; one must speak Hebrew, one should socialize, one can speak Hebrew, one is speaking Hebrew. Even the conditional tense is taught through the phrase 'If you want to learn Hebrew, you must speak Hebrew.'

Social Skills

Along with the emphasis on mastery of the language there was heavy emphasis on socialization. Israel is a country with a high degree of intimacy, where the norm for behavior is sociability. The anonymity of a New York City apartment dweller would be highly deviant in a Jerusalem apartment building. Neighbors know each other, borrow things, visit, fight, gossip. A common, revealing complaint of war widows is that they receive a suffocating amount of help and attention after their husbands' death—just the opposite complaint of American widows! This high degree of sociability was stressed throughout the lessons:

> What did you do on shabbat?
> I visited with friends.
> Good.
> And you?
> We did nothing.
> Nothing! Why? Invite somebody over—

And this went further, including the instruction of social skills.

> We don't know our neighbors, they're always out.
> Then you must write them a note, inviting them to meet you!

Sociability will combine with other Israeli values to make the good life, and this is the goal for each student who lives here. When accounts of Sabbath activities included specific, valued activities, whether religious, or secular, they were carefully explained to all the other students: the reciting of the Havdalah at the end of the Shabbat, the location of various nature and archeological sights, the tiyulim or excursions into the country to the Golan Heights or Sinai.

The content of the classes dealt mainly with the experiences of immigrants, which ranged from the disrupting to the traumatic. Over the last thirty years, Israel's immigrants have included such extremely different types as barefoot Yemenites and Russian physicists, and the diverse adjustment problems are well-known to Israeli professionals. The Yeminite needed to be shown the use of a bed and a fork and spoon, the Russian had to be introduced to the concepts of job hunting in a free society. But within the diversity there are certain universal phenomena. Immigrants almost always suffer an extended period of disruption both before and after the move, they are exhausted and confused, and as they attempt to adapt to their new culture, they experience both the heights of ecstasy and excitement, and the depths of depression. As the mental health professions have demonstrated repeatedly, small supportive groups can help individuals get through these difficult stages. The ulpan group performed this role. The teachers and students provided the supportive group and understanding of the experience, while the material in the lessons also conveyed the message 'what you are experiencing has been experienced by many others, and we all understand. Even if you have to wait in line, are treated rudely, can't get an apartment or a telephone, there are so many of us in it together . . .' The texts used in class were explicit about this:

> Adon Hasidoff arrives at the airport. He is very anxious because for a long time he has waited for this day. He must wait in line for the clerk to open his bags and his passport. He must live in an immigrant center. He also does not know Hebrew. He is not used to life in the country. But he has much patience . . . Yes it is very hard to begin to live in a new country if you do not know the language, but everyone gets accustomed, slowly. If there is patience. You must have patience.

Those who come to stay often find themselves confronted by a morass of problems, of which money, housing and employment are only a few. The social skills needed to cope with these become part of the lessons also. Proteksia, or a front-running connection, is a most popular and effective device. If one can get a person with some kind of influence (friendship, the right nationality, status, etc., or merely experience) to smooth things the problem often can be solved. So prevalent is this strategy that the ulpan office, like most offices, was full of guides and mentors when we registered, as each student had brought one along! The same strategy helped at customs, with housing, in getting the right school, or a telephone . . . In schools, every new child is assigned a student guide from his classroom to translate lessons, to help buy books and supplies. The personal element is an important factor, and even those of us in the country four or five months soon found ourselves filling this helping role for those who came after us.

One cannot always have this proteksia, and must learn new skills of negotiation. These have their own distinctive flavor. An individual who is overly sensitive may have difficulty getting past the front doors of many agencies, and so the lessons also convey appropriate methods of verbal challenge, rational argument, and even gestures. The necessity of 'savlanoot' or patience is emphasized because it is essential. But we also learn that most things are negotiable, and the bargaining of the marketplace also has its value in the office, for the bureaucracies, even with their human weaknesses, also have the strengths of being human. The teacher tells numerous stories which teach the necessary approach.

> I went to the post office at 6 o'clock. It is the 15th and I must pay my telephone bill or the service will be cut off and I will be fined. The clerk sits at her table, working. 'Closed' she says. 'But, I say, the sign out front says open until 7.' But I am closed, you see, come back tomorrow. But that is too late; I must pay tonight. I stood beside her table. 'I am closed I tell you. Would you like to see the boss? Yes, I said. I was shown into his office. I told him the story. Ah, he says, and takes my payment . . .'

Negotiation skills will be needed to deal with a multitude of problems. A murmur of understanding was shared by the class.

A Rumanian boy joins the class for several weeks, cramming Hebrew, as he must retake his last year of medical school in order to practice medicine in Israel—but there is no room in the medical school, and he feels he is forgetting the curriculum. A couple cannot find a nurseryschool for their son and may have to leave the class. Several have no prospect of jobs in their fields, and suitable apartments are hard to find. Life is expensive, and even more so after a 40% devaluation occurred simultaneously with an economic program which cut many subsidies, causing prices to shoot up. More than once, women crumbled under a sharp word from the teacher, weeping, tired of struggling with the new set of problems. But no one would have had it any different. There is one group value which pervades all discussions and that is a love of 'Eretz Israel.' The teacher asks a student to use the verb wish and he replies 'I wish peace for Israel.' She asks a Russian woman why she came.

> Because it is our land. We fought for seven years to get out of Russia. I wouldn't leave the country without my son. I told them they were crazy. I even went to the American embassy. Yesterday my son volunteered for the army; he isn't even finished with learning Hebrew yet, but he said I'm here, I want to help . . . they said take advantage of your deferment, but he said I want to go.

Another elderly woman explained,

> When I came here I wept.
> Why?
> Because it is our land.

We read poems, shared frightening news, complained, sang songs, together. This shared feeling and commitment became an increasingly powerful and cohesive bond.

The students

Spending our weeks together in this one classroom, we come to know each other very well. Our class breaks down into four major groups, each with its own special character. The Russians are dominant, and it is not just because there are so many, nor because we have all heard so much about their cause. As individuals they make such an impression: strong, vigorous, expressive and garrulous!

We are fascinated by them and their determination, and they are equally fascinated by us. "Why would a woman work if she doesn't have to?" they want to know. "How is it that young Americans are so closely identified with religion?" That we have religious schools, that children are learning Hebrew in America, that young people are as religiously active as their elders in some cases. For the Russians, "religious" was more of an absolute term reserved for a handful of aging ultra-orthodox people in an infinitesimal minority. In America, people are seeking their ethnic and religious roots. In Russia, what an advantage to lose them!

But those who emigrate are among the hard core who are far more Jewish than they could ever be Russian. Benjamin delights in discovering the Yiddish songs he knows in common with David. They grew up at the same time, fifty years ago—the one in Latvia, the other in Brooklyn, New York. When Peter joins the class, two weeks out of Russia, he immediately approaches Helena, speaking to her in English. "The Finns are unusual people. You resisted the Russians as nobody else did. There is now a town next to Finland which retains its Finnish character so strongly in spite of the Russians—it is so beautiful . . ." Helena's eyes are filling with tears. "I know. That was our town. Whenever I think of my childhood, I miss our villa so much—it was a great large villa, with so many rooms where we played, but, you know, *you* do not seem to have a Russian spirit!"

It gradually becomes clear that there is another distinctive group among us, the multinationals. And the question that makes it clear is a simple one: 'Where were you born?' Hillel, Anna and Visia had all been born in Warsaw, the two eldest narrowly escaping the tragedy of the Warsaw ghetto where almost everyone they knew perished at once. For Hillel the route out was Denmark, for Anna, South America, California and then Israel. Anna has completely lost the ability to learn a language somewhere along the line, and after eight years in the U.S. speaks an incomprehensible blend of Yiddish and English and couldn't begin to master Hebrew—although her Spanish and Yiddish, from earlier times, are excellent. Hillel and Visia, the young Polish girl who had emigrated to Denmark recently (at a time of marked anti-semitism in Poland) were outstanding in the class. Both had learned so many languages that they seemed to speak Hebrew well from the second week, with an air of complete assurance and a smooth flow of words, even though, like the rest of us, they had few words and many mistakes!

Both also spoke English fluently, without ever having lived in an English-speaking country. I compliment Hillel on his English. He laughs. "Do you know when I learned English? Fifty years ago, in school—and I never have used it, until coming to Israel this year and meeting so many people who speak it . . ." And I compliment Visia on knowing so many languages so well. "It must be satisfying to be able to speak with everyone." She replied: "I learned Danish because I had to in order to attend the university. And it wasn't so satisfying then you can be sure," she laughs.

We became very close as we persevered. Our closeness was obvious in so many ways. For example, when someone was absent there was much discussion and speculation as to why, and if this continued several days someone would be asked to try to find out why. We became like a large, sometimes quarrelsome, often very involved, family. In one little dramatic piece, arranged for the purposes of increasing fluency with some new vocabulary, Sarah played herself and she had a real problem—she was sick. Before four sentences had elapsed, everyone in the room was offering advice and this topic of concern dominated the class for two days.

A distinct identity of 'our class' as separate from all others developed, which carried over into hafsakas, and was increased by such group activities as a Chanuka party and a school trip to the Knesset. This became sufficiently strong so that when a teacher was sick and two classes combined, people were on edge and the two classes clustered in different parts of the classroom. Our class labelled the other 'immature and distracting' as they related to each other with far more joking and less studiousness than 'our own' while they expressed their sympathy to us that 'we weren't such a fun class'. All students from both classes left early on that day—the only time this happened in the three months, although they said they thought the substitute teacher was 'good and tried hard'.

Adjustment was clearly not easy for many of the students, and everyone in the class became aware of others' difficulties. One woman had undertaken the course upon her husband's death from cancer after emigrating to Israel on a stretcher in the terminal stages, and intermittently she sobbed. Several other elderly people were trying to solve the problems of settling large extended families, complete with all the educational, social and housing considerations involved—one such family even included a 100-year-old uncle as a recent immigrant! One of the multi-national refugees confided that she found it hard to be in Israel. In America she had a nice apartment, and here in the absorption Center she shares a flat with a Russian woman and her boyfriend and this is 'not nice' for her daughter to be exposed to.' 'They also drink vodka which upsets her.' Several of the women worried about the daughters—not only were they immigrants with all of the attendant cultural problems, but to be unmarried in their twenties was perceived as an additional problem in Israel, where couples marry, settle and have children young. Another woman had come without her husband, by choice, and was starting all over again with two new roles as an Israeli and as a single parent. The American olim had their adjustment problems too. Generally considered 'different' because they have chosen to immigrate in a completely free atmosphere, they nevertheless find that nothing has prepared them for the frustrations of the new language and the absence of American-style efficiency and comfort and luxuries. While young Americans often thrive on the ideological way of life and its attendant deprivations, these can be a major adjustment hurdle for older olim. The

Americans who combined immigration with retirement share the problems of both situations. As one expressed it, 'of course we eat lunch in our room—although they provide it in the communal dining room. Cooking is the absolute highpoint of our day. There's nothing else to do yet.'

The group cohesiveness is a powerful force in the lives of the students, and most particularly for those (housewives, the retired) who would have no social group to enter were it not for the ulpan.

Group Norms

Group norms develop around the behavior of students. When students are 'good', there is general approval. Almost all play the role of 'good student', coming promptly, doing homework, remaining attentive. Some seem unable to be 'good' at all. M. came to class late, if at all, and couldn't pay attention, and once even brought his 3-year-old for the five-hour day to the teacher's horror. B. refused to sit at the table and moved his chair to a corner where he opened the window wide, in spite of complaints, and swatted flies loudly. S. came an hour late until the scoldings from the teacher led him to stop coming completely. To all these behaviors, the class was outspoken in its disapproval—even lacking the words to express it. There was frowning, shaking of the head and 'tsk tsk'—apparently understood by all. Without ever being verbalized, other norms were also established and agreed upon. For example, everyone helped those few who had learning problems—whispering in the native language was tolerated only for them by the students. Everyone was restless when certain students talked too much and too long, and there was general disapproval when several shy students could not take their turns and speak at all.

Stereotyping became marked; the two college students in the class always volunteered, other students did not. The couple always spoke as a couple and the teacher ceased to call upon them as individuals.[14] The most frightened did not overcome their stagefright. The 'bad' students almost never did the 'right' thing.

Under the strain of the classroom, there were dramatic instances of regression. As one 23-year-old expressed it,

> "I never would have believed I could have regressed eight years in one week."

as she played tic tac toe with her neighbor. Others would sneak in candy or newspapers, and one man carved miniatures under the table. In tests when the teacher left the room, everyone asked their neighbor's advice.

There were numerous other pressures, especially on those who were immigrating. As one man put it, who had dropped out of two ulpans earlier, "How can I sit in kindergarten all day when I've got real problems to solve?" That more than half the class persevered to the very end was a major accomplishment. Motivation seemed more related to freedom to apply oneself than the presence of related, long-term goals. Two of the repeaters, drop-outs from previous ulpans, were immigrants who badly needed the language for coping with daily life but could find no time to study. The outstanding student was a gentile girl who had no plans for use of the language, but had acquired excellent study habits in college and had nothing distracting her from review of the lessons as she was single and knew few people in Israel.

In spite of the inevitable regressions of adults returning to a confined classroom situation role for 30 hours a week, the strong force of the class's norms and the commitment to the group goal of living in Israel and communicating in Hebrew pulled people through a major task. By the end of the class students were very close and even exaggerated this closeness. Those who were leaving traded addressses and phone numbers. When students met on the street or at concerts there was a reunion atmosphere. Those in the same language groupings visited back and forth. Graduation was a ceremonious occasion, emphasizing the importance of the accomplishment. Students contributed to a gift for the teacher—and when they were embarrassed to find how little the money bought in Israel, gave her two gifts instead. A photograph of the class was taken. Many students brought food and wine. The principal joined the class for the presentation of the diplomas, which were given out to applause. The teacher wrote her name and address on the blackboard for students to keep, thus conveying her availability in the future in spite of the formal end of the class.

The ulpan had enabled the students to master rudimentary Hebrew in both spoken and written forms, and to become oriented to their new culture. It was also extremely effective as a supportive group setting. In Israel, ulpanim serve a variety of groups. Children learn to play and study in Hebrew, professionals to work or teach in Hebrew, and recently it has been suggested that a new kind of ulpan be designed for Russian academics, including an orientation to job seeking in an open democratic society. Others interested in the field—including this author—are exploring designs for orientation groups for specific groups of olim and foreign students—which may or may not be combined with ulpanim. The potentialities of group settings for adults in transitional situations are manifold, and many are already smoothing the adjustment of newcomers to Israel.

There is an additional dimension to the international group experience which should be further explored: the opportunity it provides each participant to transcend his own culture and nationality. Individuals stripped of identity and put together on a new task quickly begin to differentiate between what is absolute in life, what relative; what is cultural in man, what universal. Crossing the world and looking back at one's own country and knowing intimately others from all over the globe and learning what it is they hope, fear and love, contribute a unique existential experience. While this kind of education clearly is not easily obtained, its potentialities for creating communication and empathy in the family of man should be explored in the coming decades as part of our search for international understanding and peace.

FOOTNOTES

[1] Aramaic term for 'custom, training, instruction and law'.

[2] Shaked, Joseph, 'Israel', in *Some Studies in Education of Immigrants for Citizenship*, UNESCO, publication No. XVI, Education Clearing House, 1955.

[3] Small group theory is set forth in numerous articles and books; one good analysis is that of *Lieberman*, Mortan A., Yalom, E. and Miles Matthew, E., Basic Books, New York, 1973.

[4] Mellah: the Jewish ghetto of the Middle East.

[5] Statistical Abstracts of Israel.

[6] Genesis; 12:1.

[7] 95% of Israeli adults over 45 years of age are foreign-born—Statistical Abstracts of Israel.

[8] 'Hebrew with Simon Tov', Ministry of Education and Culture, Instructional TV Center, Everyman's University Pilot Project, 1975.

[9] Moshavim: collective agricultural town.

[10] Banai, Margaret, *What's Cooking in Israel?*, Thomas Crowell, New York, 1972.

[11] Dolzin, Arye, on Immigrant Absorption, as quoted by the *Jerusalem Post*, Friday, November 29, 1974.

[12] For a good theoretical discussion of small group tasks, see Lieberman *et al*, *op. cit.*

[13] One couple rejected my overtures. They were the only couple out of many in the school, who had chosen to be in the same classroom (others all felt they would be able to use the experience much more fully if they separated). They always sat together, shared a book, and remained together in the hafsakas. As they were so rigidly a pair, the teacher used them to illustrate the plural throughout the course and this became the focus of a number of kindhearted jokes. When I approached them in Hebrew, the husband replied 'Hebrew is for class, not hafsaka' and yet within two weeks, whenever they had comments or questions for someone, 'outside' themselves, they frequently came to me.

[14] Bar Josef, Rivka, 'Social Absorption of Immigrants in Israel,' *Migration, Mental Health and Community Services*, edited by David, H.P., Geneva, Switzerland, 1967.

GLOSSARY

ulpan-	intensive Hebrew course
mellah-	the Jewish ghetto of the Middle East
moshavim-	collective agricultural town
ola, olim-	immigrant(s)
proteksia-	protective intervention, by someone with 'connections'; superior knowledge or power in the situation
savlanoot!-	used in the sense "be patient, relax"

SOCIOLINGUISTIC COMPETENCE AND SECOND LANGUAGE LEARNING

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Sociolinguistic Competence and the Native Speaker

In the process of acquiring language, children also acquire rules for using language appropriately:

> The ability to use one's language correctly in a variety of socially determined situations is as much and as central a part of linguistic 'competence' as the ability to produce grammatically well-formed sentences. (Lyons, 1970:287)

Ervin-Tripp identifies the individual's knowledge of how to use language appropriately as his "sociolinguistic competence" (Ervin-Tripp, 1972). Children not only acquire a knowledge of how to speak grammatically, but also of how to use their language according to the rules of speaking in their native speech-community: they learn whom to speak to, when to speak, what to talk about and which varieties or codes to use in different settings.

> Within the developmental matrix in which knowledge of the sentences of a language is acquired, children also acquire knowledge of a set of ways in which sentences are used. From a finite experience of speech acts and their interdependence with sociocultural features, they develop a general theory of the speaking appropriate in their community, which they employ, like other forms of tacit cultural knowledge (competence) in conducting and interpreting social life. (Hymes, 1972: 279)

We can sometimes observe children testing out hypotheses about how to use language appropriately: the child who asks, "Why do I have to say 'please'?" or "Why do you call Daddy 'Tony'?" or "How do I write a 'thank-you' letter to my friend's mother?" reveals a developing awareness of the sociolinguistic rules of his speech community. These rules are acquired over a number of years as a result of repeated experience of, and immersion in, a wide variety of contexts. Gleason (1973), for example, studied natural conversations in a number of families and points out that parents often supply children with a model for appropriate interaction by providing the answers to the questions they ask: e.g. "Where's your lunch-box? I bet it's inside"; or by asking another question to keep the interaction going: e.g. "Did you have a good day? It was sports today wasn't it?"

> The child really does not have to do anything but say 'yes' or 'no'—the mother is providing the whole conversation herself, and, undoubtedly, in the process, teaching him how to make a conversation and what kind of responses are expected of him. (Gleason, 1972: 162)

In multilingual contexts where two or more languages are used in different and complementary domains, for different speech functions, children will acquire sociolinguistic competence in several languages. Edelman (1971), for example, reports that six to twelve year old Puerto Rican bilinguals in New York could identify the appropriate code (English/Spanish) to be used in settings such as the home, the church and the school.

This process of acquiring rules of speaking, or rules for using language appropriately in one's speech community, continues into adulthood. "The matrix formed in childhood continues to develop and change throughout life with respect both to sentence structures and their uses." (Hymes, 1972:287) Some rules of reference and address, for instance, change as one grows older; and the types of situations in which one is required to participate generally increase over time: the rules for speaking in a public and formal meeting, or for appropriate interaction in an interview, for example, are generally not acquired until late adolescence, and some people never need to acquire these rules of use. But if a person finds himself for the first time at a formal meeting and wishes to participate, he will need to know such rules as who has the right to speak at any one time, the rules of address and reference appropriate to the context, and the complex constraints on topics of discussion which must be recognised. Some of these rules are easy to make explicit; others are much less conscious and must generally be acquired by experience of the situations in which they apply. In multilingual contexts too, the acquisition of sociolinguistic competence may involve acquiring different codes, and different rules for using these codes, well into adult life. Sorensen, for example, reports of the Indian tribes in the Northwest Amazon:

> I observed that as an individual goes through adolescence, he actively and almost suddenly learns to speak these additional languages to which he has been exposed, and his linguistic repertoire is elaborated. In adulthood he may acquire more languages; as he approaches old age, field observation indicates, he will go on to perfect his knowledge of all the languages at his disposal. (Sorenson, 1972: 86).

Sociolinguistic Competence and the Second Language Learner

The process of acquiring sociolinguistic competence, then, is normally a gradual one for the individual in his native speech community. A second language learner, however, generally lacks time and opportunity to acquire these rules "naturally". The second language teacher who wishes to help learners to acquire rules for using English[1] appropriately as well as grammatically, and who wants to enable students to understand the social as well as the referential meaning of exchanges, needs to discover ways of facilitating and accelerating this acquisition process. Where students have only a limited amount of time to devote to acquiring English there is clearly a need for methods and materials which will enable students to increase their competence as quickly as possible. There is not yet a great deal of material available which aims to develop foreign students' sociolinguistic competence in English. This situation is being remedied however, as material is developed based on the information provided by sociolinguistic models which make explicit the rules of speaking English appropriately (e.g. Brown & Gilman, 1960; Schegloff, 1968; Sacks, 1973).

Sociolinguistic Interference

In trying to develop a foreign student's sociolinguistic competence, it is important to bear in mind that the adolescent or adult second language learner is by no means sociolinguistically naive. He has already acquired the complex sociolinguistic system used in his native speech community. In learning how to use and interpret the sociolinguistic rules of English he must develop an awareness of areas in which the sociolinguistic system of his native language differs from that of English. In other words the notion of "sociolinguistic interference" is useful in enabling the language teacher to focus on areas where "misperception and misanalysis" may occur (Hymes, 1972). As Ervin-Tripp points out, "A contrastive analysis of formal rules, in combination with a theory of social learning, would allow specification of what will happen" (Ervin Tripp, 1969: 104).

The need to learn to pronounce new sounds, to acquire new stress patterns, vocabulary and grammatical structures is only too apparent to any second language learner. The need to acquire new sociolinguistic rules of speaking and interpretation, however, may not be so clear, since superficially the differences between any two sociolinguistic systems may not seem very great. In some situations the rules of use may appear similar enough to develop a false sense of security in the learner. This problem is compounded by the sociolinguistic rule in English which imposes "very strong constraints on teaching adults to behave," as Paulston points out (1974), and perhaps also by "a feeling that it is chauvinistic to correct immigrant-like behavior" (Paulston, 1974; 353).

Nevertheless it is clearly the teacher's responsibility to provide feedback to students concerning the appropriate or inappropriate use of English, since the students themselves are likely to be unaware of many of their errors in this area. (See, for example, exercise 1.) Paulston (1974) has discussed the problems which arise when a student "applies his native rules of speaking to the target language, rules which may imply a very different social significance." An excellent illustration of the dangers of directly translating "rejoinders" from one language to another is provided by Dobson (1974: 22).

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[1] Clearly much of what is said applies to teaching any second language. Our own experience is in TESL, however, so from this point on, reference will be made only to English for exemplification purposes.

> When I was first learning Turkish, I did not know the proper Turkish rejoinder for expressing surprise, so I translated the English expression 'Really?' literally into Turkish. I continued to use this form until a kind person told me that my rejoinder implied actual doubt as to the truthfulness of my interlocuter's statement. Immediately I set about learning the correct rejoinder for 'I'm surprised!'

Paulston (1974) also points out that "it is easier to keep one's linguistic codes separate than one's social codes as one often is not aware of the social codes on a conscious level until they are violated." The teacher, by taking the responsibility for pointing out to second language learners potential violations of sociolinguistic rules, may help to improve the learner's sociolinguistic skills.

Using previous and present experience to develop competence

We are more concerned here, however, with suggesting methods of developing sociolinguistic competence in English by the use of exercises and class discussion, than with correcting inappropriate behaviour when it has occurred. In the absence of adequate descriptions of the (two or more) sociolinguistic systems involved, the choice of areas on which to concentrate in the classroom must proceed initially on the basis of a teacher's experience and observation of students both in the classroom and outside of it. Adult students bring to second language learning an ability to introspect which can be extremely valuable. Hence the learner's sociolinguistic competence in his native language can be regarded from this point of view as an asset rather than a liability. Adolescent and adult learners have acquired rules for speaking appropriately in at least one speech community, and the teacher can make use of this sociolinguistic competence to facilitate contrastive analysis of sociolinguistic systems in the classroom.

Material which focusses on areas of potential sociolinguistic interference enables the teacher to stimulate introspection and discussion of the different sociolinguistic rules used in each of the languages. One method of doing this for example is through role-play; there are many sources of interesting and useful exercises which enable one to focus on the development of sociolinguistic competence (e.g. Kettering, 1974; Nation, 1975; Selekman, 1973). A variation which has proved particularly rewarding is Savignon's suggestion that students enact a communicative situation in their native language and then watch the same situation enacted by English-speakers (Savignon, 1972). This procedure provides students with an opportunity to make explicit sociolinguistic differences they have not previously perceived and to observe features they had not realised were socially meaningful.

In attempting to increase a student's sensitivity to the social meaning of English, classroom exercises can also be used to focus on particular examples where the social meaning of an exchange is not obvious to a person from a different cultural background. Once the purpose of such exercises is grasped, students should be encouraged to bring their experiences in the second language community into the classroom for discussion. They will become increasingly aware of instances where the social meaning of utterances or exchanges in the second language does not match that of exchanges in their mother tongue in a parallel context, and where they suspect that they misinterpreted (or misanalysed) a situation. In some cases this may have been clear to them as the conversation proceeded; in others they may have realised in retrospect that something had been awry in the exchange; and in others of course they may never become aware that they misinterpreted the social significance of a conversational exchange, or that they were regarded as rude, comical or over-demonstrative by their fellow-participants.

Another aspect of sociolinguistic competence which second language learners often need to develop is more creative than interpretive. Many foreign students lack the confidence to initiate or contribute anything substantial to a conversation. The native-speaker is often expected to carry the conversational load while the foreign student replies to his questions with the minimum of information, thus providing him with little encouragement to continue the conversation. In order to improve this aspect of sociolinguistic competence students need practice in contributing material to a conversation, in providing information which their conversational partner can pick up and develop, and in taking responsibility themselves for developing a conversation (see Brown & Holmes, 1976).

Styles

In devising exercises aimed at developing sociolinguistic competence we made an initial distinction between receptive and productive sociolinguistic competence (c.f. Troike, 1969). For receptive purposes we considered that some familiarity with at least three styles, (varieties or registers) of English was desirable: a formal style, a consultative or neutral style, and an informal (casual or colloquial) style.[2] The distinct linguistic characteristics of such styles are not always easily identifiable, as Denison, for example, has pointed out:

> In monoglot communities, where diatype differences are register differences, we are, in spite of such formal marks of register delimitation,still left with a certain methodological awkwardness in our search for correlations between register and situation-category in communicative events, for each must be used to help fix the bounds and characteristics of the other. (Denison, 1968: 581)

We found that by discussing both the social and the linguistic characteristics of different styles or varieties of English, students could learn to identify samples (both written and spoken) as formal, consultative or casual. (c.f. Friend, 1969) The social variables we made use of were variables such as the features of the setting (including place and time), characteristics of the participants and their relationship (including role-relationship, relative status and degree of friendship), and the association of topic and function with different styles or varieties of English (Ervin-Tripp, 1964). The distinction between "manifest function" (referential meaning) and "latent funtion" (social meaning related to speaker's aims) was emphasized. It is clearly an important one for second language learners, since even native speakers may find it causes problems in some situations. If the latent function is not clearly signalled this "may lead to social embarrassment because of differences in interpretation by speaker and hearer" (Ervin-Tripp, 1964).[3]

For production purposes we decided to concentrate on the consultative or neutral variety. For most situations this variety is the safest and most appropriate one for a foreign student to use. The English our students speak when they arrive in New Zealand often verges on the formal in many of its features, and sounds rather laboured in many situations. One student, for example, on being asked "Could you shut the door" replied "I will do so," which, although grammatically correct, sounded much too formal in the context. Many students clearly have some degree of control over a formal variety already. On the other hand it seems inappropriate to teach them to use an intimate variety since they are unlikely to need it for the short time they spend here, and if they decide to stay longer they will acquire it more adequately in their interactions outside the classroom, than within the classroom where it is scarcely appropriate. There is also the danger, as Camutaliova points out, that "what is intimate for the native speaker may sound improperly familiar when used by a foreigner" (Camutaliova, 1972: 171).[4] It seems a terrible waste of time, then, to teach productive competence over a wide range of English styles. Productive competence in a neutral or unmarked variety, together with some knowledge of a casual style, will generally be both adequate and appropriate in the majority of situations in which students will be involved.

Our broad framework, then, can be represented by the following diagram. The shaded areas are those we have concentrated on in our teaching.

Styles

	Formal	Consultative or Neutral	Informal (Colloquial or Casual)	Intimate
Socio-linguistic Competence Receptive	▨	▨	▨	
Productive		▨	▨	

Exercises

The exercises which follow are examples of material used to develop the sociolinguistic competence of 55 adult foreign students on a summer course in New Zealand. They came from the following countries: Bhutan, Burma, Cambodia, Chile, The People's Republic of China, Egypt, Germany, Indonesia, Iran, Japan, Korea, Malaysia, Nepal, New Caledonia, Peru, the Philippines, Poland and Thailand. None of them had lived in a monolingual English-speaking community before, and they all expressed a desire to improve their ability to understand and communicate effectively with native speakers both in "transactional" encounters (e.g. shop-keeper - customer) and informal "personal" encounters (e.g. student - students) (Blom & Gumperz, 1972). We divided the students into groups of eight. In each group we tried to ensure that there were at least two people of any nationality represented, so that it was possible to contrast sociolinguistic rules in their language and in English. This composition was also necessary in order to practice Savignon's contrastive role-playing activities (Savignon, 1972).

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[2] Hymes (1964) suggests that every society has at least three style levels: formal or polite, colloquial, and slang or vulgar. Joos (1959) identifies five levels (frozen, formal, consultative, casual and intimate). The three levels we have emphasized seem to us the most clearly distinguishable and most useful to second language learners.

[3] The terms manifest and latent function are useful theoretical concepts but it was not necessary to burden students with them. For their purposes the terms referential and social meaning were quite adequate.

[4] These comments are equally applicable to non-standard dialects. The use of non-standard forms of English by foreign students is likely to be regarded by native speakers as presumptuous or as a marker of lack of education. It may even lead to ridicule. Standard forms are unmarked in the majority of situations in which foreign students participate.

The exercises illustrate the three main aspects of sociolinguistic competence on which we decided to concentrate.

(1) Exercise 1 was used to introduce the students to some of the terminology we felt would be useful in discussing sociolinguistic rules and to demonstrate the considerations relevant to making an appropriate response in a variety of contexts.

(2) Exercise 2 focuses on receptive sociolinguistic competence. We concentrated here on developing students' ability to recognize the many different functions of questions. Brown & Holmes (in press) provides another exercise to develop receptive competence which focuses on extending students' ability to interpret latent function: i.e. where the referential content of a remark differs from its social meaning.[5]

(3) Exercises 3-6 were used to develop students' conversational skills in a variety of situations. We were particularly concerned that students should learn to adapt their English to the situation, and that they should develop the ability to participate fully in casual exchanges and accept some of the responsibility for developing the conversation.

Exercise 1: Appropriateness and politeness

Introduction

When something is *appropriate* it is right or suitable for a particular situation. We speak of appropriate behavior and appropriate dress. Speech is verbal behavior so we can have appropriate or inappropriate questions or statements in a conversation. Whether language is appropriate or not depends on the situation.

When a person is *polite* it shows he has good manners and considers the feelings of other people. We speak of a polite boy, a polite remark, or a polite manner. We usually try to be polite but the words used to be polite vary with the situation.

Appropriateness and politeness therefore both vary with the *situation*. We can classify situations in many ways: a simple classification is

1. *formal*
2. *neutral* or *consultative*
3. *informal* (*colloquial* or *casual*)
4. *intimate*

We can use these adjectives to describe the situation or to describe the *style* of speech. The neutral style is probably the most useful and the informal (colloquial or casual) style the next most useful.

Using English appropriately often involves judging how polite you need to be in a situation. Here are some situations and some possible responses. Using the words *underlined* in the introduction, discuss the responses.

Part A

Situation 1: You want to borrow a dictionary from your friend.

Questions

(a) May I borrow your dictionary?
(b) Lend me your dictionary, will you?
(c) Can I have your dictionary?
(d) Are you using your dictionary? May I borrow it for a moment, please?
(e) Would you be so kind as to lend me your dictionary, William?

Situation 2: Your lecturer or teacher or boss asks you to dinner. You cannot go.

Question. Would you be able to come to dinner next Tuesday?

Answers

(a) No, I can't.
(b) I'm very sorry, I can't, that's the night for my extra English class.
(c) I'm already booked for Tuesday.
(d) Thank you for asking me; but I cannot come that night.
(e) Sorry mate, not that night.
(f) Thank you but I cannot attend.

Situation 3: You are offered some coffee by the mother of one of your friends.

Question. Would you like a cup of coffee?

Answers

(a) Yes, thank you.
(b) I sure would.
(c) Yes, please thank you.
(d) Yes, thank you very much.
(e) I would indeed be very grateful for a cup of coffee.

Situation 4: You find you can't get home for dinner and you are staying with a New Zealand hostess. You need to tell her. You ring up and say:

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[5] Mortimer (1974) suggests further useful methods of sensitising students to the implications of utterances.

(a) I'm not coming back for dinner tonight.
(b) I'm afraid I won't be home for dinner tonight.
(c) I hope you won't mind if I don't come in for dinner tonight.
(d) I'm sorry to ring so late, I've been asked to the Wilsons so I won't be back for dinner.
(e) I regret I am unable to be present for dinner tonight.
(f) Mrs. Wilson has asked me to dinner so I hope it's alright if I don't come back. I'll be home about ten.
. . . . or you don't ring her at all.

Situation 5: You are visiting someone's house and you want to use the phone.
You ask:
(a) May I use your phone, please?
(b) Where's your phone?
(c) Would you be so kind as to let me use your phone?
(d) Can I use your phone?
. . . or you go to the phone and use it without asking.

Part B

Here are three different situations. Decide which of the four styles is most appropriate and then make up some possible responses.

Situation 1: You are in a shop. Yesterday you bought a shirt and today you want to change it because it is the wrong size. Ask the salesman to do this for you.

Situation 2: You are carrying a heavy box. A good friend of yours says "Can I help you?" You think it's easier to carry it by yourself. Refuse his offer.

Situation 3: You think the shop-assistant has given you too little change after you have bought something in a shop. Let the shop-assistant know.

Comments

A few students tended to select the very formal answers in every situation because they seemed the most polite. This gave an opportunity to discuss the complex relationship between politeness and appropriateness and to point out the distancing effect that over-politeness often has.

Situation 1 produced a discussion about "may/can, may/might, can/could."

Situation 3. "Yes, please thank you" was considered acceptable by some students and a discussion followed about "please" and "thank you." Some students, because they know they can say "yes please" or "yes thank you" do not know which one to use and so use them both at once.

Situation 4. Some students did not recognize "regret" as a formal world. Choice (f) led to discussion about whether you should tell your hostess when you will be home.

Situation 5. The discussion included questions about when you should offer to pay for a phone call in a private home.

Part B gave the students an opportunity to perform.

Situation 1. Many students did not realize that they were in the wrong and therefore did not use "I'm sorry", which would be considered an appropriate conciliatory opening.

Situation 2. Most students said "No" and then expanded, but almost no-one used the word "manage" or gave a positive answer such as "It's quite alright, I can manage thank you".

Situation 3. This gave an opportunity to mention non-verbal communication. If the customer looks at the change then at the shop assistant and the again at the change, his meaning is clear.

A number of students did not know "*change*" was this meaning.

Getting the right degree of uncertainty was difficult for some, and so the word "abrupt" was introduced as we encouraged students to avoid appearing too abrupt.

Exercise 2: Functions of questions

Introduction

The foreign language learner often thinks of questions as structures that are predominantly concerned with seeking information. The question form, however, performs many other functions in ordinary conversation. If the learner can interpret the intention of the question it will help him to respond. In a real conversation, of course, intonation and context play an important part in helping the listener to guess the questioner's intention;[1] but, even without these, an exercise such as this gives an opportunity for students to think about the nature of social interaction. The exercise may also serve as a way of giving practice with some items of vocabulary: e.g. permission, confirmation, influence, sympathy, etc.

Exercise

Here are some questions, and some reasons why people ask questions. Discuss the questions with your neighbor and see if you think the same way about the reasons why a person might ask each question and about what might be an appropriate response.

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[1] Dickinson and Mackin (1969) provide some excellent material for practice in producing and interpreting the meaning of different intonation patterns in conversation.

Reasons for asking questions:

To get new information.
To get sympathy.
To get something repeated.
To get an explanation.
To ask for confirmation.
To offer help.
To get some advice.
To give sympathy.
To try to influence
someone.

To ask for a reason.
Just to be polite.
To make a request
To ask for help.
To ask to be listened to.
To make a suggestion.
To get an opinion.
To ask for permission.
To ask someone to
agree with you.

The Question

1. How does it work?
2. What does that mean?
3. How is your mother?
4. How are you?
5. Do you think I should ring him now?
6. Why are you crying?
7. Would you tell me once more please?
8. Do you think it's going to rain?
9. Shall we go to the zoo?
10. Have you had any news from home yet?
11. Can you unscrew this?
12. Do you think this suits me?
13. Did you say ten o'clock for the train?
14. Are you busy at the moment?
15. Lovely weather, isn't it?
16. Why didn't John come?
17. Will you open the window please?
18. Will you excuse me, please?
19. Do you really think so?
20. Shall we dance?
21. Why does he always look so sad?
22. Where does he live?
23. Do you feel better now?
24. Why don't you share it with your sister?
25. Is there anything else I can do for you?
26. Do you like hot curry?
27. Do you think President Kennedy was a good statesman?
28. Why don't you go to bed now?
29. Do you think I'll ever get better?
30. Why did you do that?
31. May I have the red one?
32. You agree with that, don't you?

Comment

The exercise was presented with the questions listed in one column and with two empty columns alongside, one headed "The purpose of the question," and the other headed "Possible answer." The students first discussed the exercise in pairs and then there was class discussion. Finally each student filled in the columns for homework and these were checked. It was obvious from the written work that a few students were still having difficulty in deciding the function of some of the questions as these examples illustrate:

	Question	**Purpose of the question**
e.g.	Do you think it's going to rain?	Suggestion
	Will you excuse me, please?	Explanation
	Do you like hot curry?	To offer help
	Lovely weather, isn't it?	To try to influence someone

Other students were using very formal language.

		Response
e.g.	Why don't you go to bed now?	I'll do so.
	Do you feel better?	Yes, I do feel so.
	Would you tell me once more, please?	Of course it will be a pleasure to make clear what I've already said.

The question "Do you think President Kennedy was a good statesman?" produced the most varied suggestions about its function. Answers included: to get some advice, to try to influence someone, to get confirmation, to get information, to get an explanation, to get an opinion. One student showed particular skill in his suggested reply to this question: "In some ways he was and in some ways he wasn't."

There are other ways of doing similar exercises; for example many questions do require some sort of information. It is possible to think of different sorts of information and make another exercise aimed at making students aware of what kind of information is being sought.

Different sorts of Information	**Examples**
Definition	What's a porcupine?
Opinion	Which one of these do you like?
Classification	What's your job?
Explanation	How do you use it?
Comparison	What's the difference between yours & mine?
Reason	Why can't I go?
Identification	Could you tell me which of these buses goes to the railway station?
Description	How will I know it's the right house?
Evaluation	Is it a well written book?

Exercise 3: Dealing with "or" questions

Introduction

Questions involving alternatives are often difficult for foreign students to answer. Some students answer "yes" when they mean "both," and few students realize that "or" questions often allow more freedom of response than is apparent from the form of the question. In some situations it is necessary to answer by choosing only one of the alternatives (e.g. No. 1 below); but in many situations there are more than two answers (e.g. No. 3 below).

If you do not want either alternative you can say "neither", but if you say this, it is usually polite to explain why: e.g. "Do you want a bath or shower?"

"Neither at the moment, thank you, I have just had a shower."

Part A

Discuss the kind of situations where these questions might occur and suggest possible answers. First do them orally. After discussing possible answers with the class you could write answers to be corrected. Try to decide how many possible alternatives there are—two, three or many.

1. Would you like a lift or do you want to walk?
2. Did you say four kilos or fourteen?
3. Do you want to come to the cinema or are you going to bed early?
4. Will you have tea or coffee?
5. Do you want your tutorial time to be eleven o'clock on Tuesday or three o'clock on Thursday?
6. Will you have brown or white bread?
7. Do you want a smoker or a non-smoker?
8. Have you finished your study for today or do you have more to do after dinner?
9. Is it urgent or can it wait?
10. Do you think it's more important for students to learn to speak English well or to read English well?
11. Can you do it by yourself or do you need my help?
12. Will you sit down or do you prefer standing?

Part B

Now try to make up some "or" questions where there are only two possible answers.

Comments

Part A

Question 1: The students realized there were only two sensible alternatives, although there were many ways of phrasing the reply. Some students asked about "hitch-hiking": who does it? what are the unwritten rules for it?

Question 4 led to a discussion about the situation in which you can say "Neither thank you, can I have fruit juice?" and of the differences between a public eating place and a private home and the effect the setting has on the style of language used.

Question 6 led to a discussion about whether you could say "both" and how you would refuse both of them.

Question 9 prompted a useful discussion about a likely situation for the question, e.g. hospital, dentist, repairs to various things, getting in touch by phone.

Number 1, 7, 9 and 12 were thought to have only two answers in most situations.

Part B

When the students tried to make up questions where there were only two possible choices, they began to see how difficult it is to think of such situations and so became more aware of the wide range of available responses to what had seemed closed questions. Some students could not think of any. Here are some that they asked each other:

Is it male or female?

Do you want to send it by airmail or surface mail?

Have you got a job or are you still looking for one?

Has the matter been decided or is it still under discussion?

Are you single or married?

(But here it was realized that some forms give four alternatives: single/married/widowed/divorced.)

Will you travel first class or economy class?

The purpose of this exercise was to demonstrate to students how much flexibility there is in conversation. They found they could often provide alternatives to what had seemed a simple choice and began to see the usefulness of this skill in a conversation: e.g. a good way of changing the topic of conversation is to use these openings to give new information or to ask a new question:

e.g. A. Will you have brown bread or white?
B. I am dieting at the moment so I won't have any thank you. I find that dieting isn't very easy in New Zealand because the food is so delicious.

Exercise 4: Two consecutive questions

Sometimes a speaker asks two questions in the one sentence, or two questions one after the other, without pausing. He expects you to give two or more bits of information in your reply.

(a) Try answering these questions. Let one member of the class ask the question to three others and then discuss the replies.

(b) Discuss the sort of situations in which these questions might occur.

1. Have you been to town? What did you buy?
2. Was the examination hard? Which questions did you answer?
3. Did the police come, and what happened then?
4. Do you think it will rain? If it does, how shall we alter the arrangements?
5. Is she still in the hospital? How much longer must she stay there?
6. Are you ready? Are you sure you want to come?
7. Will you pay for your ticket now and do you need hotel accommodation?
8. Did you really enjoy that film? What was so good about it?
9. Have you had a good sleep? Do you feel better now?
10. How did you break your leg? And how long must you keep it in plaster?

Comments

Some students found it hard to answer both parts of the question; they tended to answer only the second part. Occasionally this was acceptable but generally two pieces of information were needed. The students said they wished that people wouldn't ask questions this way, but they recognized that such questions were quite common.

Question 1 brought out a difference between rural and urban people: e.g. one student replied "I was there for two days."

Question 4 gave an opportunity to teach "alter the arrangements" and "postpone."

Question 5: The use of the word "still" was important as the implication of its use was not always appreciated.

Question 6 produced an interesting discussion about possible situations and alternative responses.

When the class had finished following the instructions, they did the exercise again orally, working quickly, taking turns to ask the question to only one person. Finally they were given a new sheet and answered the questions for homework.

Exercise 5: Awareness of differences according to whom you are speaking

This exercise is similar to one suggested by Friend [1969]. We were particularly interested in collecting examples from the students that could be used to discuss appropriate styles of English in different situations. The students were therefore asked to do this as a written exercise.

You have just lost your purse or wallet with $50 in it and you tell first your closest friend, then the warden of the hostel to explain why you can't pay your board, and then the police. Write out three appropriate conversations.

Comments

The materials produced were used to draw attention to differences when speaking to different people. One student finishing the conversation to a friend, and to the warden, wrote "bye." In conversation with the policeman however he wrote:

Policeman: Bye, see you, nice weekend.
Student: Bye, see you, nice weekend for you too.

Because this example was available, the teacher was able to draw attention to the fact that the leave-taking for the policeman was probably more suitable for a friend.

Another student, when reporting the loss of money to the warden of the hostel, said, "I am going to inform you that last night my purse was lost in my room." This is probably far more formal than a native speaker would be in a similar situation, partly due to the use of the performative which is mistaken for a polite formula.

Many students did not give enough specific information to the police about date, time, size, colour, name, address etc. This example from a student's script was used to discuss how and when to give factual information.

Policeman: What size is your wallet?
Student: Not so big.

These are only three examples of the use that was made of the written conversation to illustrate points for teaching about appropriate behavior. The exercise can be thought of as preparatory to role play. When the students have done several similar exercises, they are able to handle role play situations and consequently, one hopes, real situations, much more competently.[7]

Exercise 6: Comments and information leading to conversations without questions

The first two parts of this exercise rely on ideas from Dobson (1974). Giving new information or commenting on a topic are important parts of conversation.

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[7] Coles & Lord [1976] provide some excellent examples of speech routines and useful formulas for speech functions such as introductions, compliments, leave-taking etc. They include a variety of styles and can be used as an illustrative source for students. Kettering [1974] is also an excellent source of material on speech functions and is generally very helpful in providing material for the development of sociolinguistic competence.

Part A

Each student makes one statement about a given topic. Each student should make a different statement, preferably one he has never made before. Work quickly round the class. Try this with:

__________ my shoes __________
__________ sleeping __________

Part B

The student this time is required to make a two-statement comment to a whole sentence: e.g.

A. I must go to town before the bank closes.
B. I could lend you some money if you don't want to go. I was paid yesterday.

Let students make the initial statement and choose who will make the next two statements on the same topic.

1. I don't know how to find the library.
2. I wish I knew how to speak Russian.
3. I have lost the key to my room.
4. There were only ten people at the meeting last night.
5. I have heard that you play the guitar.
6. It is very hard work looking after small children.

Part C

Now work in pairs, make up a conversation where each person speaks at least six times. Do not have any questions in the conversation. Be prepared to demonstrate your conversation in front of the class. You can use rejoinders in your conversation if you need them. Here are some examples, there are many others.

That's too bad!
Really!
What a pity!
Don't be silly.
I agree with you.
I'm surprised.

Remember the listener shows he is listening by using these phrases, by eye contact and by facial expressions.

Do not use too many rejoinders as they do not develop the conversation.

Each person should contribute equally to the conversation.

Comments

Part A proved particularly good for shy members of the group as everyone had to take part. With only eight members of the class it was possible to go round three times quite quickly before students had exhausted spontaneous comments about their shoes. The students were able to give comments at their own level of proficiency so the answers ranged from simple sentences such as "My shoes are black" to "There is a hole in the sole of my shoe so the water comes in."

Part B was useful as practice in resisting asking questions. It can be argued that it is unnatural not to allow questions, but the students are so wedded to the idea that conversations consist of only questions and answers that these exercises are justified because they provide practice in another strategy for developing a conversation.

Part C produced plenty of talk in the room and after the students had practiced in pairs the performance to the class was given. These performances gave the tutor the opportunity to comment on any features that were unusual or incorrect. The students were told they could write out and hand in a conversation for correction if they wanted to. Here is an example of the sort of work produced. One student came from Nepal and the other from Japan. It may be a little formal but it represents a real exchange of ideas—a useful social skill for students to acquire and one which requires a degree of confidence in the second language.

A is a Nepalese, B is a Japanese.

A. Life in Wellington is different from that in Nepal.
B. Indeed! It is also quite different from that in Japan.
A. I think it may be due to cultural differences.
B. You are right. The basic difference in New Zealand life from Asian countries is due to the difference in culture.
A. The cultures of Asian countries differ much among themselves.
B. Our culture is affected by the Chinese culture. So, I think, there is no basic difference in culture between your country and mine.
A. No, there is a great difference in our cultures. Nepalese culture is much affected by Indian culture.
B. Really! But our religion is the same — Buddhism. And Buddha was born in Nepal.
A. Of course he was born in Nepal. But Hinduism is our national religion, although Buddhism originated in Nepal.
B. It is true. But I must point out, there are no Buddhists in New Zealand. This is why you feel the difference of life between New Zealand and your country.
A. Religion is not the only difference; the family structure is also different between New Zealand and your country.
B. I agree with you. And besides these things the social and economic structure is also different for historical reasons.

CONCLUSION

Teachers of English as a second language are becoming increasingly aware of the need to develop, not only the linguistic competence of their students, but also their sociolinguistic competence. The ability to correctly interpret the social meaning of an utterance and to produce appropriate utterances in a variety of social contexts is best acquired through experience of different social situations. This may require spending a great deal of time in the speech community whose language is being learnt. Where this is not possible teachers face the difficult task of providing opportunities for developing sociolinguistic competence within the time restrictions and sociolinguistic constraints of the classroom situation.

The exercises presented in this paper were devised in order to help adult students of English as a second language to speak and understand English in the wide variety of different situations they would encounter outside the classroom. They allow for varying levels of linguistic competence by providing opportunities for all students to use what they know, regardless of the size of their vocabulary, the complexities of the grammatical structures they have mastered, or the accuracy of their pronunciation. The emphasis has been on developing an awareness of the importance of using language appropriately and interpreting meaning accurately, rather than on acquiring new words and structures or improving pronunciation. In developing sociolinguistic competence, however, new words and structures are inevitably introduced and pronunciation practice is provided, since the exercises for the most part require students to use the spoken language. They also focus students' attention on important aspects of communication which are often neglected in the classroom.

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POPULAR CULTURE IN CROSS-CULTURAL PERSPECTIVE

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> **With the exception of the Hollywood motion picture, it is hard to think of an instance in which those cultural products of a country which are actually its most characteristic and widely diffused are also the ones by which foreigners know it.**
>
> Ithiel de Sola Pool
> Foreword to *Japanese Popular Culture*[1]

The academic study of popular culture is a result of widespread interest in the effects of mass media and the popular arts following World War II. Its beginnings were not concerted in any single field of specialization, but took shape within a wide range of disciplines in both the social sciences and the humanities, in forms ranging from communication effects studies to literary criticism of popular novels and the development of an aesthetics of cinema. During the past thirty years, popular culture study has come to be viewed as the study of mass media, entertainment, and leisure-time activities. Appearing in widely-scattered form across the spectrum of the American university curriculum, popular culture study has found expression in various courses dealing with film, televison, radio, the popular press, (including news, the "best-seller," pulp fiction, magazines, and comics), popular music, and the advertising establishment. New journals featuring popular culture, as well as more established academic publications, are making it their policy to include popular or "current cultural" topics such as case studies dealing with celebrities, media purveyors and empires (such as those of Hearst, Annenberg, Walt Disney, major film studios, and television networks); media events (such as fads, sports spectacles, and notable television specials—e.g. Elvis Presley's satellite-transmitted performance in 1973); and political or sociological analyses of the context and role of various popular culture phenomena.

Underlying Assumptions and Implications

Growing up as it has in America, popular culture study is imbued with American values, particularly the notion that electronically-based mass media are the indispensable foundation of popular culture. Consequently, extending this American study beyond U.S. borders is proving to be difficult because of the many subconscious or unexamined assumptions and beliefs which underlie it: that popular culture is somehow Western, must occur after the late 18th century in a democratized society under majority rule, in a capitalist economic system, and have large populations concentrated in urban areas, mass education and literacy, technical sophistication and leisure, and a large middle class with diversified lifestyles (the description formulated by Russel Nye, a leading proponent of popular culture study, in his encyclopedic text, *The Unembarrassed Muse*).[2]

This predisposition, which automatically excludes most of the world by its de facto description of Europe and America, creates a gap between popular culture study "as we (Americans) know it" and the emerging interest in popular cultures of other countries. This new interest takes the shape not only of global popular culture study (mainly, so far, extensions of the American popular influence on Europe), but includes also the International Style—the supracultural life-styles expressed in the convergence of cultures on a global scale—and in much smaller measure, comparative popular culture.

Americans seem to want to reserve for themselves the ingenuities and mythic design of the mass media, and to think of the rest of the world (outside Europe and European-based countries) as running directly on pure psychic energy without material mediation—without a popular culture. This simply indicates that Westerners are accustomed to thinking about popular culture in a limited manner and with certain images in mind, as an organization of personalities, events, artifacts, and institutions based on Western artistic, political, and technical concepts.

One of the most pervasive underlying beliefs about culture in the West is that the literary/artistic realm of life is separate and distinct from that of the scientific/technological, a popular theme which has been under discussion in intellectual circles since the beginning of the Industrial Revolution. In summing up this tradition of "the two cultures," in his article of the same name, C.P. Snow gave a concise locution to the conventional defensiveness on the part of men of general culture (and cultivation) against those of the narrower, specialized disciplines of the sciences:

> I believe the intellectual life of the whole of western society is increasingly being split into two polar groups... Literary intellectuals, at one pole—at the other, scientists...Between the two a gulf of mutual incomprehension—sometimes (particularly among the young) hostility and dislike, but most of all lack of understanding.[3]

When this model is correlated with the gap between "high" and "popular" culture (that is, understood as a distinction between unique and mass-produced artifacts), popular culture emerges as an outgrowth of the scientific rather than the artistic world, since it is assumed to be produced by technology—i.e., applied science. This alignment of the popular with the scientific is at the root of the literary intellectuals' distrust and distaste for popular forms.[4] Ironically, the printing press and the publishing industry which have played a major role in the transmission of culture for hundreds of years, as well as establishing the American preeminence in the literary arts are, of course, also to be counted among the legions of applied science.

By a further extension, this cultural scheme also divides East and West, allying the "Timeless East" with the humanities, in opposition to the scientific-technical, industrialized West. Although this severely simplistic split has begun to fade as a basis for contrasting cultures and subcultures, the stereotype is remarkably persistent as a compelling categorization. Such unexamined assumptions about differences between East and West help to explain why the current framework of popular culture studies is too restrictive for global study. An expanded concept of popular culture could allow it to be seen as a broad fabric extending geographically to cultures other than the developed, electronic, literate, and urbanized, and historically backward through history to pre-Industrial societies. All of our academic disciplines are actually Western-centered and encounter problems of bias (all the worse for being invisible and impalpable) when they attempt to analyze non-Western cultures. (see paper by Singh, this volume).

The sort of difficulty posed by the conventional Western attitude is well illustrated by a prominent American popular culturalist attending an international conference on cross-cultural popular culture held at the East-West Center. Although his interest in popular culture was interational, to his dismay, he found himself the object of great hostility, directed at him mainly because of his restrictive Western definition of popular culture—one which most Americans accept uncritically. Before his "Asian encounter," this scholar evidently found no cause to think that he would be posing controversial issues. Asians at the conference felt particularly distressed and insulted by his vision of global popular culture as electronic and Western; the emerging International Style, given these terms, is commercial, middle-class, youth-centered, electronic, urban, and affluent, with a modicum of non-American content—in other words, strongly American in content and feeling.

Many of the conference participants came from countries which are struggling to become not more international but more self-consciously nationalistic. As new national identities, many are just inaugurating collections of folklore, heroes, traditional artifacts, rituals, and customs. For these nations, their pre-colonial heritage, which we would call folklore, is much more important to them than "progress." Their feeling is that this heritage must, in fact, be assiduously protected from the electronic incursion from the West, as in the People's Republic of China's cautiousness about Western media imports (almost their only indulgence is in Eastern European film). What Third World nationalism is trying to accomplish is directly antithetical to the Western popular culture mode. For popular culturalists, one answer to this discrepancy is to ignore those cultures until they catch up and start producing their own mass media instead of borrowing ours; the other, more justifiable, answer is to extend popular culture study *outside* the customary American limits.

Broader Horizons

In order to accomplish this broadening of perspective, popular culture scholars can become more sensitive to their own definitions and attitudes, and can direct their attention to other cultures to observe their richness for research. This is not to propose the model of the neotraditionalist described in *The Lonely Crowd*, who, as much as he appreciates Eastern tranditional life for its beauty, quaintness, and exotica, bases his personal life on the most avant-garde of Western models. Evidently, different standards are being applied in this case to what one values for oneself as compared to what one exhorts as being good for others. As Riesman et al explain:

> [Neotraditionalists] seem to want to deny to others the privileges of modern society which, however, they themselves take as a matter of course . . . They are apt to share the outlook of the city planner who said that he thought the ideal communities in America were to be found among the rural Negroes of the deep South and the French Canadians of the Quebec villages. It was disclosed later in the conversation that his own friends were scattered over two continents.[5]

While it remains for dedicated scholars to broaden popular culture study to include a global appreciation, in fact the conventional definitions and concepts of popular culture are inadequate even in the West. American popular culture, considered the world's most advanced and futuristic, also contains a strong mixture of "pure" mass culture and traditional culture, challenging the many detailed distinctions which have been attempted between folk, popular, mass, and elite. For the most part, the "eternal values" of human life are alive and well all around us, but in a hidden dimension—made invisible by the selective perception of "good taste." The inherent age-old functions of many of our most despised mass culture forms often don't register as such because they are masked behind contemporary "disguises."

For example, an appreciative study of the Disney theme parks (Disneyland and Walt Disney World) can yield an entire set of meanings which extends far outside the restrictive arena of meanings usually stipulated for popular culture.[6] Not only are these "plastic" and "electronic" amusement centers important as the popular culture capitals of America, but for all their technological innovation and expertise, they are reviving many "traditional" and even "lost" features of American life which can be experienced in these specialized environments by their millions of visitors. Enclosing a number of separate "worlds," the parks actually recreate a series of vanishing aspects of traditional American life: the small town (with its Main Street); the pleasure drive; integrated "vernacular" architecture; handcrafts and decoration; pedestrianism, the town square, and the sense of personal interaction with other people and with the surroundings associated with festive community occasions. In another, more familiar instance, the urban shopping center or mall performs much the same function as the small-town Main Street, now fast disappearing from American life, by providing a comfortable, leisurely atmosphere free of noise and pollution, abounding with plantings and sculpture, which features an easygoing pace, frequent "resting spots," and mall-side cafes and restaurants to foster sociability and relaxation.[7]

Reinstatement of these old-fashioned values in Disney's futuristic style provides a good lesson in perception and open-mindedness. We must guard against allowing the guise and historical context of artifacts—especially if they are contemporary—to obscure their essential social and psychological roles in our culture. They may be new versions of an old idea, or old versions of a modern one. Now that many are drawn to the antiquarian in succeeding waves of the nostalgia epidemic, there is an increasing collective wish for things to return to the way they were in "the good old days." However, perhaps many older practices and institutions continue to operate widely, but in altered forms which make them difficult to classify and identify as extensions of their traditional counterparts. An ability to see through these modern disguises may offer a new sense of the term "transitional sensibility," applied here to a psychic mobility or imagination in Western man rather than to the re-orientation of self in the throes of development as experienced by Third World "transitionals."[8]

This insight can enable us to see popular culture as tradition, and vice-versa. Basically, it can provide a framework for looking at Asian culture from a new perspective. Extending this ability further can help to unveil the continuing roles of popular forms within Western culture long after these forms have branched off from their original neutral places in pre-Industrial society into either arcane elitism or into low-brow media. For example, vestiges of live theatre (in the sense of unscripted, spontaneous acting) still flourish, both in avant-garde style in New York playhouses and as "happenings," and as "big-time wrestling." Folk music, which in traditional settings is value-free, has become high-brow as "field recording" in the ethnographer's collection, or low, as in Country and Western.

On a grander scale, television, like traditional religion, provides a guide to mores and behavior as a common national "shared experience"—the largest in human history.[9] In the perennial "violence controversy" implicating television in the instigation of anti-social behavior, intellectuals (and others) generally hold that all but education channels and programming are debased and corrupting. But perhaps more importantly, television is aimed at the lower middle class and below.[10] Most people approve and support the *idea* of "shared experience" in general—but apparently the manifestation has to fit the right profile or recognition doesn't occur. In the same way, the ancient Greek pantheon of gods and goddesses is extremely elitist now. Its modern approximation (as suggested by Gore Vidal in *Myra Breckenridge*) is the Hollywood star system. As another example, amateurism, highly prized by the neotraditionalist in foreign settings, continues on in American popular culture as NBC-television's *Gong Show* and in other off spring of the original *Ted Mack Amateur Hour*. This type of amateurism is ridiculed, however, because its style is thought to be abrasive, outrageous, and definitely lower-class.

Can these historic forms and "universal" impulses in modern shape avoid the destructive dilemma of a forced choice between archival seclusion or a cheap, debased popularity? This is a vital problem for the future cultural unfolding of America and Asia alike, not only to grasp what is happening to older forms under pressure of "future shock," but of course, to consciously chart whatever cultural development can be designed and molded through policy.

Moving outward from the strict definition of popular culture as linked to the mass media, popular culture study has recently begun to concern itself with the ordinary substance of everyday life, as distinct from the forms of elite or high culture. The new concern with the behavior and materials of daily life—the banal and mundane, ordinary, usual, and tacit—has been introduced in the works of numerous sociologists and anthropologists of the last generation, including Riesman, Garfinkel, Denney, and Goffman.[11] These scholars began to look seriously at the informal, leisure aspect of American life as an alternative to work and institutional studies (the "central" concern of life) which had traditionally made up the bulk of sociological research. The sociology of everyday life, the "history of the inarticulate," urban folklore, urban anthropology, image studies, and mass media history and analysis as well as subjects and methods drawn from numerous other fields as diverse as geography and kinesics, are all now contributing to the growing tradition informed by an interest—although not generally recognized per se—in popular culture study.

No discipline has ever laid any exclusive claim to the study of popular culture. Most popular culture artifacts, integrated as they are within the organic ecology of the culture at large, defy analysis by any single field. Disciplines such as psychology, social psychology, anthropology, sociology, and folklore have all recently begun to "work in" toward popular culture through awakening interest in urban leisure America. In their exploration of the parameters of "informal life," what these scholars are studying is popular culture. Ideally, popular culture research will begin to take its place as a needed focus within every aspect of learning. The current emphasis on popular culture as a field in itself seems useful mostly to call attention to the necessity for the view of culture it can offer as an unorthodox yet fundamental academic awareness.

Popular culture study is also concerned with helping to introduce and reinforce a new methodology of involvement. Popular culture scholars, in order to understand their subjects, depend on a capacity to understand society from the inside by participating in and feeling its immediate and rich empirical patterns, best encountered by experience, accompanied by intellectual analysis. This model of the active researcher grows out of a natural affinity of the scholar for his subject matter which is constantly part of the texture of his life. In popular culture, the observer and the observed share the same world. As students of popular culture begin to see in television and film as well as in cocktail parties and shopping centers deeper social and psychological meanings, so they can begin to see the participants in these events as active shapers of the environment, with intuition, intelligence, and creative volition, rather than in the conventional model of passive victims. This more humanistic version of "mass man" can be extended cross-culturally. The specific ways in which individuals act and react within each cultural setting, however, are unique. The reintroduction of popular culture forms as an expression of the surface texture of the mythic ideals for every culture, especially in the area of the popular arts, proposes to ally popular expressions with the more humanistic search for myth and symbol in the liberal arts.

Cross-Cultural Perspectives

Unfortunately, the newly emerging view of popular culture as an organic whole which can be understood only within the context of a holistic concept of culture is seldom extended to cross-cultural studies in popular culture. In general, Asian culture continues to be known to the West largely through a limited set of artistic, religious, and political studies. In the case of Japan, appreciations in English are top-heavy with treatments of Japanese art by Westerners. Religion—Hinduism and Buddhism—has been the dominant source of interest in India. Studies of Chinese political history are generally geared to official policy and aristocratic (or power) regimes rather than to popular movements or the new school of the "history of the inarticulate" in social history—even in contemporary commentaries on the People's Republic. These interests give Westerners a free-association set of concepts and images of Asia drawn from aristocratic and scholarly sources, which are treated as exotic adjunct to Western histories and typologies, often without an understanding of the holistic cultural world-view which produced them. Ithiel de Sola Pool has described the contrast between popular and elite sources of culture learning:

> The culture of Japan has long intrigued the West. We have been charmed by beautiful prints, scrolls, dancers, costumes, and objects of everyday use. On Japanese films and minor arts there is a vast literature in English . . . But when one culture is thus introduced to another culture, that which is introduced is apt to be high-brow culture. In Japan as elsewhere some gulf exists between the creative art of the sophisticates and the popular arts which flow through the mass media. It follows that in the process of international communication the material which is not "worthy" of being translated gets lost from view and is unknown abroad . . . Comic books, popular songs, cheap periodicals pass unnoticed and unrecorded by those who chronicle the ways of the world.[12]

In general, the state of cross-cultural analysis is arrested at the elitist stage. Many scholars in the now developing field of socioliterature feel that literature is a kind of documentation or barometer of cultural character and values. Many of them are convinced that in fact cultures can be best understood through a study of high literature, based on Matthew Arnold's famed opinion that literature is the highest achievement of civilized man and the most refined expression of his particular sensibility. For example, Japanese novels are often pointed to as one accessible mirror of Japanese values and beliefs for the Westerner. And in one study of the use of literature as cultural artifact, Heinrich Boll was selected to represent mainstream German popular culture. De Sola Pool comments on the use of such works as educational resources, using Japan as his example.

> The many books about Japanese fine arts or the translations of first-rate Japanese literature published in the U.S. are of course valuable means toward international cultural understanding. But they are not an adequate clue for an American reader who is interested in the life of the common people in contemporary Japan. For that we need other evidence.[13]

This methodology for understanding other cultures through their finest novels, drama, and poetry can be compared to looking solely at the works of Faulkner, Fitzgerald, and James as the groundwork for appreciating American culture. Compare this spectrum of writers and the quality and rarified reputation and readership of their works to the actual record of the public taste in fiction: *Peyton Place* has long been the best-selling novel in America (since 1956) and the single most popular genre is the detective story, led by writers like Erle Stanley Gardner, Ellery Queen, and Mickey Spillane.[14] The use of high literature as a guide to an entire culture should be an extremely conditional approach, especially given the evidence that many societies are either pre-literate (in the Third World) or post-literate (in America, where the major leisure pursuits are television and sports).

Some new currents in cross-cultural study along the lines suggested are evident. New interest in such aspects of the long and rich cultural histories in Asia as acupuncture and herbal remedies, martial arts and the martial arts film, the Japanese wood-block print and Chinese opera is accompanied by increased sensitivity to the importance of the interaction of the popular cultures of the world. These new awarenesses are based not on the usual elitist models used by Western scholars—which have held that art, religion, and government are the most important things for the West to know about Asia—but have a broader base, consisting of two traditionally divergent concerns: 1) the study of traditional cultures through anthropology and folklore, and 2) electronic mass media (the study of the diffusion of Western popular culture).

The works of anthropologists—Ruth Benedict, Margaret Mead, and Edward Hall (especially in *The Silent Language* and *The Hidden Dimension*)[15]—supply good models for beginning cross-cultural popular studies. Other studies are also pursuing an interest in the everyday and ordinary: linguistics, kinesics, sociability, communication, cultural geography, social psychology—with special areas devoted to time sense and spatial relationships. One example of this trend is a recent film by John Nathan *(Full Moon Lunch)* (1977) which provides a sensitive insider's view of urban life in Japan. The film works inductively by detailing the everyday activities of a "downtown" Tokyo business family but attempts no conclusions. This approach is quite different from the usual practice in such documentaries of using the subjects as illustrations of preconceived cultural "truths." The result of this approach is an open-minded film that successfully captures the rich texture of a "typical" family's everyday life. Interaction of cultures seen in popular terms (rather than in the customary political and artistic terms) is one direction being taken by students of global popular culture, including but going far beyond the conventional analytical focus: the diffusion of American popular culture, thus far limited mainly to Europe.

For Americans, much of the interest in the newly emerging International Style of cultural convergence is in tracing the American popular culture influence in its rapid spread through the world since World War I. But with the record of this diffusion yet uncharted, people's responses to American products as they diffuse to other countries is yet unclear. What, for example, is the effect of Disney films in Japan? How do they influence Japanese impressions of the United States and of American life and character? How can these attitudes be isolated and analyzed? What will be the impact of the new Disneyland now in the planning stages for Tokyo? The "Disney Effect" is difficult to separate from others, and even more difficult for an American to discern. One almost has to be bicultural to make sense of diffused popular cultures. The work of a few pioneers in cross-cultural studies, such as Walter Ong[16] and John and Ruth Useem[17] suggests the exciting possibilities of studies which incorporate a more complete picture of culture. Recent articles on Japanese-American relations, such as Harold Isaacs' and Akira Iriye's mutual image studies[18], evidence more interest in contemporary popular Japan by Americans. Results of these observations so far suggest that diffusion of electronic media into traditional cultures has brought about an interaction and cross-breeding of traditional cultures, giving birth to a proliferation of new cultural sensibilities featuring the International Style as a prominent example.

American academic prejudices against popular culture, which operate even in popular culture research (usually in the form of defensiveness), are not universal. Most Asians do not separate their popular and "non-popular" cultures to reflect the biases associated with the "two culture" split between the arts and sciences and within the "class system" of elite, folk, mass, and popular forms. As an illustration, the Japanese woodblock print, developed early in the 17th century, "the world's first art for the masses,"[19] is not an object of disparagement anywhere, in East or West. Most of the Japanese popular arts have been perfected for centuries and have built up a refined tradition (dignified by time) which our popular culture hasn't had time to develop—and with rapid cultural change, may never develop. The diffusion of these popular arts in the West and Western electronic exports to the East are both part of the same international process. And of course, with the Chinese detente, the study of Asia is taking on a new immediacy for the understanding of world culture.

Despite these advances, cross-cultural studies continue to suffer from a general failure to recognize that different cultures may be built upon radically diverse worldviews—differing religious and moral assumptions, and corresponding mythic frameworks. At the first Comparative Popular Culture Research Seminar held at the East-West Center in 1974 (a comparative study of Eastern and Western heroes), participants found that Lord Raglan's classic list of 22 requisite steps in the heroic saga, which have been considered the key to comparative hero study in the West, are quite culture-bound and can't be applied across the board to Asian heroes. The same is true for aesthetics; the "atavistic," "natural," associations of certain colors (white for purity, black for death, red for vitality) are often different or contradictory in Asian cultures; for instance, white is a funeral color throughout East Asia. Many "universal" concepts and symbols, which we unconsciously accept as having one meaning for all people (in art, particularly) are not universal at all; global meanings can't be assumed for products of particular cultures.

Even when an event or artifact appears to have a general acceptance around the globe—the most widely-recognized commercial objects in the world are Coca-Cola and Mickey Mouse—we must ask, "Does this object mean the same thing to people in Japan? Egypt? India? as it does to Americans?" Popular culturalists are used to relying heavily on a living familiarity with their native cultures to make intelligent connections between cultural artifacts and the underlying mythic structure—that is, the cultural worldview as perceived behind the surface texture of these artifacts. But when that structure is unfamiliar, we must be especially cautious and curious. What is the meaning of Marilyn Monroe for Asians? Abraham Lincoln is one of the heroes most frequently included in Asian textbooks: how many Asians are textbook material for Americans? How does Lincoln's meaning change from America to China? To Hong Kong? To Japan? One clue is the Chinese and Japanese respect for intellectual life; Lincoln's self-styled education is admired in Asian countries, while American popular heroes are notoriously anti-intellectual. Could Lincoln's intelligence be central for Asians and peripheral—if not a "minus" in his character—for Americans?

Gandhi's reputed celibacy after the death of his father provides a further example. Westerners assumed that Gandhi's asceticism was something that matched his high political and spiritual idealism—a sort of lofty universal. They were willing to let it go at that without asking too many questions. According to Erik Erikson's psychobiography of Gandhi,[20] the truth was both less and more particular than that. While Gandhi was making love to his wife, during a respite from caring for his father, the old man died, his "last words" and blessing a mystery. The blessing of a son by his father before death is a crucial part of the Hindu concept about the continuance of life through the generations, and the guilt one must bear for violating this sacrament is excruciating ("A blot which I have never been able to efface or forget"—Gandhi),[21] enough to make Gandhi renounce sex forever. This background is important, and can't necessarily be inferred either from Western values or from our preconceptions about Indian values, but presents a third, unanticipated interpretation.

In order for Western students of popular culture to hope to understand Asian cultural artifacts, they need to incorporate the insights of cross-cultural experts. As a starting-point, the Western outlook is offset by at least two other major worldviews. According to philosopher Huston Smith[22] the world can be divided according to three basic philosophical structures: the Western fascination with analyzing the natural world; the Indian development of the psychological, inner man; and the Chinese concept of man as a social being whose identity derives from the group. Although starkly simplified, this scheme challenges the classic conception of a dichotomy of spiritual East and material West, offering a more complex understanding of cultural orientation. The above example from the life of Gandhi should show the dangers of interpretations about the "mystic" East. We tend to divide the world up into scientific and spiritual, with the temporal values, including Popular Culture, on the Western side, and the arts and religions in Asia. Note also, that under this conception all of Asia is considered a single, monolithic place. In fact, of course, Asia can be subdivided by cultures into many distinct cultural styles and histories, just as Europe can be.

This simplification also ignores the other side of the various cultures of both East and West, the spiritual (or at least metaphysical) meanings which are as much a part of popular culture as are the purely material ones. As one instance, the films of Alfred Hitchcock (still considered "light entertainment") are in one sense an attempt to deal with the problem of evil and the modern struggle against spiritual chaos in a secular world which appears to have renounced its spiritual traditions. An holistic approach to popular culture uses but goes far beyond psychology in locating and describing "deeper" meanings for such popular icons and events as Clint Eastwood movies or shopping centers or the roller derby.

The "revolution of rising expectations" in Asia certainly sets its people in a material context, and since World War II, the U.S. has been instrumental in appraising the overwhelmingly pragmatic problems of Asia. In addition, the U.S. reinvigorated the "revolution" by creating needs which can't be met by struggling economies, as Sukarno accused with reference to America's exportation of refrigerators to Indonesia.

It has been more than a century since anthropologist Edward Tylor defined culture as "that complex whole which includes knowledge, beliefs, arts, morals, law, custom, and any other capabilities and habits acquired by man as a member of society."[23] Even when this idea of culture as a unified whole is acknowledged intellectually, American scholars and those influenced by American scholarship evidently have difficulty putting it into practice, even in such a "democratic" study as popular culture. For these students of the culture, matters of cultivated taste, in the form of discrimination against certain subjects, persist. When popular culture is sensed as a way of reaching beyond the surface texture of a culture to grasp its essential beliefs and values—on a historical continuum including all cultures—it is possible both to see the West as running on "eternal values" and also to see Asia as a secular, lively, earthy, pragmatic, *popular* place;[24] a healthy antidote to the bulky stereotypes which have prevented a more extensive and more complex picture of both hemispheres.

Conclusion

These are some of the problems of definition and attitude facing scholars of popular culture who want to compare cultures or to trace popular culture diffusion. However, solutions to these problems, even if they become available, will be restricted until the already-existing framework of American popular culture is expanded to incorporate a wider definition that will allow popular culture study to explore and explain world culture and the relationships between cultures. A sense of direction for the most fruitful popular culture research across cultures must perhaps come from the non-Western world.

NOTES

[1] Ithiel de Sola Pool, foreword to Hidetoshi Kato, Ed., *Japanese Popular Culture* (Tokyo: Charles E. Tuttle, 1959), p. 9.

[2] Russell Nye, *The Unembarrassed Muse: The Popular Arts in America*, Introduction (N.Y.: Dial Press, 1970).

[3] C.P. Snow, "The Two Cultures," *New Statesman* (October 6, 1956).

[4] A thorough discussion of the short-sightedness and limitations of the "two-culture" dichotomy can be found in Susan Sontag's essay, "Our Culture and the New Sensibility," (1965), in *Against Interpretation* (N.Y.: Farrar, Straus & Giroux, 1966). She points out that underlying the conception of the arts as engaged in a death struggle against the sciences is the notion that the arts are static, while science changes; without regard for the many recent developments in the arts which show certain scientific attitudes about inquiry and specialization.

[5] David Riesman, Nathan Glazer, and Reuel Denney, *The Lonely Crowd* (New Haven: Yale U. Press, 1950), p. 278. The authors add that "Here we find the classes attempting to force 'roots' upon the masses, just as the Dobuans try by magical incantations to keep their yam tubers in place!"

[6] For this and other examples of this approach to popular culture using the "Disney Enterprise" as an example, see my "Disneyland and Walt Disney World," in Reuel Denney, Ed., *Dictionary of Popular Culture*, to be published.

[7] Ralph Keyes, *We, the Lonely People. Searching for Community* (N.Y.: Harper and Row, 1973), pp. 114-141. In his section on the shopping center as a recreation of small-town environments, Keyes explains, "The shopping center, like other enclosed climate-controlled malls, was the most tranquil and pleasant environment I've ever found within suburbia. Like the town square of old, it provided the opportunity to promenade among the familiar faces of those living within."

[8] Daniel Lerner, *The Passing of Traditional Society: Modernizing the Middle East* (N.Y.: The Free Press, 1958).

[9] Russell Nye, *The Unembarrassed Muse*, p. 407.

[10] In *The Hidden Persuaders*, Vance Packard explains the prime audience for advertising as the 65% of the American population contained in the lower middle class (white collar workers, tradesmen, and some skilled workers) and upper lower class (skilled and semiskilled workers), using Lloyd Warner's *Social Class in America* (1948), which made class distinctions based not only on the conventional basis of wealth but also on consumption and sociability patterns. (Vance Packard, *The Hidden Persuaders*. N.Y.: David McKay Co., 1957, pp. 114-116.)

[11] For examples of this approach, see David Riesman et al, *The Lonely Crowd* (New Haven: Yale U. Press, 1950); Harold Garfinkel, *Studies in Ethnomethodology* (N.J.: Englewood Cliffs, Prentice-Hall, 1967); Reuel Denney, *The Astonished Muse* (Chicago: U. of Chicago Press, 1957); Irving Goffman, *The Presentation of Self in Everyday Life* (Garden City, N.Y.: Doubleday-Anchor, 1959), and also *Relations in Public: Micro-Studies of the Public Order* (N.Y.: Basic Books, 1971); Jack Douglas, Ed., *Understanding Everyday Life* (Chic: Aldine Publishing, 1970); Ellen Steckert and America Paredes, Ed., *The Urban Experience and the Folk Tradition* (Austin: U. of Texas Press, for the American Folklore Society, 1971); America Paredes and Richard Bauman, Eds., *Toward New Perspectives in Folklore* (Austin: U. of Texas Press, for the American Folklore Society, 1972); and Roger M. Keesing and Felix M. Keesing, *New Perspectives in Cultural Anthropology* (N.Y.: Holt, Rinehart, and Winston, 1971).

[12] Ithiel de Sola Pool, foreword to *Japanese Popular Culture*, p. 9.

[13] Ithiel de Sola Pool, p. 10.

[14] Russel Nye, *The Unembarrassed Muse*, pp. 54, 268. "One of every four books published and reprinted in the U.S. every year is a detective-mystery." (p. 268).

[15] Edward T. Hall, *The Silent Language* (Greenwich, Conn: Fawcett, 1959), and *The Hidden Dimension* (Garden City, N.Y.: Doubleday, 1968).

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[20] Erik Erikson, *Gandhi's Truth* (N.Y.: W.W. Norton & Co., 1969), pp. 128-130.

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[22] Huston Smith, "Toward a World Civilization," *Center Diary* (Santa Barbara, Calif: Center for the Study of Democratic Institutions, Vol. 17, March-April, 1967).

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TEACHING THE TEACHERS OF HAWAIIAN CHILDREN: TRAINING AND CONSULTATION STRATEGIES

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The Kamehameha Early Education Program (KEEP) is a research and development program whose mission is the development, demonstration, and dissemination of methods for improving the education of Hawaiian and part-Hawaiian children. KEEP operates a four-classroom (kindergarten through third grade), 110-pupil research and demonstration school, and has investigated effective methods for disseminating its research findings to teachers in the public schools since its foundation in 1971. The purpose of this paper is to review and integrate the series of KEEP dissemination/consultation projects which were conducted during the initial period of KEEP's operation (1971-75).

Objectives of KEEP

A number of anthropological and psychological studies reported that Hawaiian youngsters are labeled "Underachievers," noted for problems of low motivation, behavior management in class, or poor academic performance (for example, Gallimore, 1971; Gallimore, Boggs, & Jordan, 1974; MacDonald & Gallimore, 1971; MacDonald & Tanabe, 1973). The majority of these Hawaiian and part-Hawaiian children were educated in regular public schools under the State Department of Education (DOE). KEEP was established to find ways to improve the educational achievement of these Hawaiian children by assisting, and not competing with, public school programs (Tharp & Gallimore, 1975). Information generated by KEEP research is to be tested and disseminated to public schools via effective consultation channels. The investigation of these channels is one of the major concerns of KEEP research. The official agreement between KEEP and the DOE provides an operational framework for KEEP's consultation efforts.

Although the purpose of KEEP is to improve overall educational achievement of Hawaiian children in public schools, given the sizable number of Hawaiian/part-Hawaiian children, direct service is not feasible. Using teachers as mediators to reach the target student population, however, provides an effective alternative to individual case consultation (Tharp & Wetzel, 1969). KEEP faced the task of evaluating various means of influencing the behavior of the mediator-teacher. The task demanded a theoretical and operational framework which would enable KEEP to test various strategies in the Hawaiian school community.

The Triadic Model of Consultation

The triadic model of consultation proposed by Tharp and Wetzel (1969) and modified by Tharp (1975) analyzes services that are not offered directly by the consultant, but through a *mediator*, a change agent who influences the *target* by using various means of social influence. (See Figure 1). The consultant's primary responsibility lies in influencing the behavior of the mediator who in turn influences the target. This model has been applied extensively in consultation services across the United States (for example, Deno, 1975; Dollar & Klinger, 1975; Fo & O'Donnell, 1974, 1975). KEEP has used this model to lay the foundation for the Demonstration School and to train in behavior analysis those teachers who are in continuous contact with children in the classroom. The major advantage of this triadic model is that "it incorporates the influence of mediators who have a continuing relationship to the child, and it offers a vehicle for teaching mediator-teachers skills of continuing utility" (Tharp, 1975:138).

If the mediator-teachers learn effective skills via the influence of their consultants, it is expected that the impact of such skill learning will be reflected in the behavior of the students. During the past decade, a host of studies have provided convincing evidence that teachers can learn behavioral techniques to influence children's behavior in school (for example, Buckley & Walker, 1970; O'Leary & O'Leary, 1972).

Evaluation research in the field of teacher training has achieved some success: For example, the literature indicates that microteaching (Allen & Fortune, 1966) is generally more effective than traditional teaching (Borg, 1972; Hall & Copeland, 1971), or if equally effective, far more efficient (Kellenbach & Gall, 1969). However, this method "scaled down in time and the number of students" is used in lab setting and is thus constrained in its application and generality beyond the particular setting in which teachers are trained (Wahler, 1969).

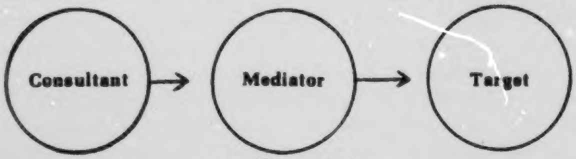

Figure 1
The Consultative Triad

KEEP's solution to this issue can be found in the very structure of the KEEP Demonstration School which is modeled as a regular DOE classroom to facilitate generalization. The teachers of KEEP are expected to adopt three co-existing roles: *teacher, consultant*, and *researcher*. Within the KEEP Demonstration School, these Educational Specialists are the target of in-house teacher training and the training effectiveness is evaluated by examining the change in their behavior. They are trained in their own classrooms by their supervisors in the effective use of various skills for behavior analysis of children. After being trained in their roles as teacher/consultant/researcher, the teachers to KEEP then become consultants to public schools to help DOE teachers in various programs. Thus, within KEEP, the Educational Specialists are trained to perform as consultants.

KEEP dissemination/consultation research, therefore, is composed of two essential components: (1) training of KEEP Educational Specialists; and (2) consultation to public school teachers in Hawaii. Both consultation and training use the same means of social influence (Tharp, 1975), but they differ in the basic social contract. In training, the relationship is between the consultant and the target, while in consultation the social structure is triadic, with the consultant influencing the mediator and the mediator influencing the target. The differences may be described as a *direct* versus an *indirect* relationship between the consultant and the target. Within this model, researchers can carefully analyze the relevant social environment and study lines of social influence. In short, the triadic model attempts to make the most of the people who are available and their social influence.

Description and Analysis of Projects

The target population for KEEP's services are the 35,000 Hawaiian and part-Hawaiian children in the public schools of the state. The first segment of this population to be intensively studied is located in an urban area of Honolulu, and is characterized by high population density, low socio-economic status, and low school achievement in Hawaiian children. The children are bi-dialectical, with Hawaiian Creole English ("Pidgin") as the preferred code. Even though they live in an urban area, many features of tranditional socialization practices continue (Tharp & Gallimore, 1976). KEEP engages in curricular, linguistic, cognitive, ethnological, and psychological research, presuming that features important to education lie in each of these domains.

However, because motivational issues are salient in the judgments of Hawaiian educators, an initial focus of KEEP has been to develop a system to increase the incentive value of school activities. Based on previous work in psychological

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[2] Figure taken from R.G. Tharp and R.H. Wetzel. *Behavioral Modification in the Natural Environment*. New York: Academic Press, 1969, p.47.

anthropology (Gallimore, Boggs, & Jordon, 1974), it was determined that a rich diet of success experiences in school is necessary for the young Hawaiian child, and that this success experience can best be provided by social, rather than material, markers. Thus KEEP has emphasized positive teacher acts—praise, patting, hugging; and by making these acts contingent on positive student behaviors. The KEEP teachers maintain a remarkably high rate of appropriate, work-oriented behavior by the children—up to 90% (Tharp & Gallimore, 1976).

KEEP publications expressly report these social reinforcement strategies are not sufficient in and of themselves to guarantee academic learning. Indeed, curriculum revisions, taking into account the linguistic and cognitive particularities of the culture, are needed to insure good performance in learning to read. As of this writing, such a curriculum has been developed and its dissemination will begin immediately.

This curriculum is based on a series of studies in which various forms and strategies of training have been explored. The studies reviewed in this paper have concentrated largely on training and consultation to increase teachers' use of positive social reinforcement. The assumption of the KEEP strategy is that the methods used to teach this behavior will be applicable to training other teacher behaviors as well.

Of course, the use of contingent social reinforcement is not restricted to Hawaiian children. A host of studies have demonstrated that contingent positive social reinforcement is effective in increasing incentive to improve performance (for example, Baer & Wolf, 1968; Becker, Madsen, Arnold, & Hall, 1967; Hall, Lund & Jackson, 1968; Kaufman & O'Leary, 1972; Zimmerman & Zimmerman, 1962). KEEP has investigated various alternative methods to train teachers in the effective use of positive attention, especially praise. These methods may be called "means of influence."

Means of Influence

Psychology has always been concerned with ways to influence the behavior of other humans. Tharp (1975) argues that all people use the same means of influence in various social conditions, and that, applied in a more limited range, both the consultant and the mediator use the same means of influence. For any teacher training and consultation, then, an investigation of various influence techniques assumes primary importance.

1. *Didactic instruction.* Instruction is one of the most common techniques used in teacher training. Typically, trainee-teachers are given lectures and reading materials that cover the basic elements of the field to be taught. Research indicates, however, that specific instruction *per se* does not lead to reliable change in teacher behavior (Madsen, Becker, & Thomas, 1968). Despite such findings, didactic training continues to be used in teacher training (Cossairt, Hall, & Hopkins, 1973). KEEP has used didactic training as a *part* of teacher training and consultation in order to impart theoretical knowledge of basic operant principles in education.

2. *Discrimination.* Effective teaching requires accurate observation of student behavior and an ability to respond to different behaviors. Teachers' behaviors influence consequent student behavior; for example, when teachers give praise contingent upon the desired behavior of students, they encourage those behaviors. Therefore, the teachers must learn to discriminate the desirable from the undesirable, both in their own and in their students' behaviors. After being exposed to definitions and categories of target behaviors, teachers are trained to acquire observational skills and to record the occurrence of target behaviors. The observation method supplies evaluation data in behavioral intervention (Bijou, Peterson, Harris, Allen & Thomas, 1969).

3. *Modeling.* Research conducted within a social learning theoretical framework indicates that complex behavioral patterns can be learned vicariously by observing other persons exhibiting those responses (Bandura, 1965, 1969). Applications of modeling procedures to teacher skill training have demonstrated that they are effective alternatives to more traditional procedures (Koran, Koran & MacDonald, 1972; Orme, 1966; Rutherford, 197[illegible]; Sloggett, 1972; Wagner, 1973; Sloat, Tharp, & Gallimore, in press; Speidel & Tharp, in press). Bandura (1969) argues that the response acquisition process can be facilitated by the provision of appropriate models. Modeling is the only behavior change technique that sets and demonstrates specific behavioral criteria to the observer (Tanaka-Matsumi, 1976), and it is particularly useful in training teachers to acquire behavioral skills.

4. *Guided practice.* This includes direct coaching, direct intervention and all other interventions which are done *in vivo* with the assistance of a guide (Bandura, Blanchard, & Ritter, 1969). Rule (1972) found that direct intervention was more effective than videotape feedback in teacher training. KEEP has applied this method using demonstration teachers (Speidel & Tharp, in press) and the supervisor of trainee-educational specialists (Sloat, Tharp, & Gallimore, in press) as direct

coaches in training teachers in contingent positive attention.

5. *Feedback*. Fuller and Manning (1973) reviewed extensively the types and effectiveness of feedback, and pointed out that video playback has been one of the most popular means of giving feedback to the trainee. Research on feedback, however, has been inconclusive: Some studies have found that feedback was effective as long as the feedback procedure was in effect (Cossairt, Hall, & Hopkins, 1972), while others found no constant effects of feedback in effecting change in teacher behavior (Rule, 1972; Rutherford, 1973). This inconclusiveness is perhaps due to confounding variables, such as presence or absence of goal setting (Fuller & Manning, 1973; Locke, Cartleyel, & Keppell, 1968). Giving explictly stated performance criteria and then feedback has produced highly positive results in behavior change (Kazdin, 1974; Saudargas, 1972; Sloat et al., in press). These studies seem to indicate feedback is effective when it is combined with goal setting, or a performance criterion.

In recent years, the self-recording or self-monitoring technique has been applied in behavior change programs (Thoresen & Mahoney, 1974). At KEEP, videotape playback of the individual teacher's classroom behavior is critically examined. In viewing their tapes, teachers focus on specific behavioral categories defined prior to self-examination.

All of the influence techniques reviewed above have been used by KEEP in teacher training/consultation. The overriding concern of the research efforts has been directed to maximizing the effectiveness of training/consultation in the classroom proper. What follows is a careful examination of how this concern and purpose of KEEP were attained, and whether or not it was done effectively. There are two patterns of teacher training/consultation. First, KEEP in-house teacher training will be reviewed and second, KEEP dissemination/consultation projects with public school teachers in Hawaii will be examined.

KEEP in-house Teacher Training

First Phase. The triadic model of consultation requires training of consultant-teachers (Educational Specialists) within KEEP before any dissemination consultation program is instituted in the community. So, the series of program was designed specifically to train a professional to function as teacher, consultant, and researcher in educational settings (Speidel, Tharp & Gallimore, 1975). Training of Educational Specialists as *teachers* began with observation training, in which teachers learn to discriminate among various teacher and student behavior. Behavioral categories were developed and videotape recordings were used extensively. Self-examination of videotaped teaching sessions was followed by consultation with the Project Coordinator.

Training of KEEP Educational Specialists as *consultants* involved three roles: a consultee, a trainee-consultant, and a trainer-consultant. During this phase of training, different people acted different roles to develop models for professional consultation, and the triadic model served instrumental to this development.

Training of KEEP Educational Specialists as *researcher* was conducted by combined practicums and seminars guided by empirical principles of human behavior (Matheson, Bruce, & Beauchamp, 1970; Holland & Skinner, 1961). The training of teachers in these practicums, and seminars on learning and experimental principles, helped them develop research designs within their own classrooms and apply those principals in everyday educational practice.

These initial teacher-training programs during the 1972-74 phase served as a pilot for further investigation of relevant dimensions of teacher training. Guided by this pilot work, KEEP launched a semester-long resident teacher-training study in the fall of 1974.

Second Phase. Five new Educational Specialists were trained to use praise effectively during a series of six one-week inservice workshops over a period of 16 weeks (Sloat et al. in press). The training was given within the theoretical context of operant learning principles in the educational setting (Ackerman, 1972; Buckley & Walker, 1970; Hall, 1972; Krumboltz & Krumboltz, 1972; MacMillan, 1973; Meacham & Wiesen, 1969). The six components of the training included: (1) didactic instruction; (2) modeling and role playing; (3) videotype feedback; (4) direct coaching; (5) feedback; and (6) feedback with goals. Each of the training procedues was designed to last for one week (five class days), preceded and followed by a week of regular teaching without training. Data were collected daily by independent observers using behavioral codes developed prior to the training. The main behavioral codes were academic praise, management praise, rule statements, commands, desists, and verbal negatives, such as threats or scolding.

All teachers were observed daily, and a unit of a 15-minute observation period was divided into five-second intervals. If any of the targeted behaviors occurred during an interval, the symbol of the behavior was recorded once on the data sheet. Inter-observer reliability was established at r=.8 or better. For the duration of the training, the teachers were not informed of the specific nature of the data being collected, nor were they informed of their performance until the fifth training condition was introduced.

Results of this training for each individual teacher are illustrated in Figure 2.

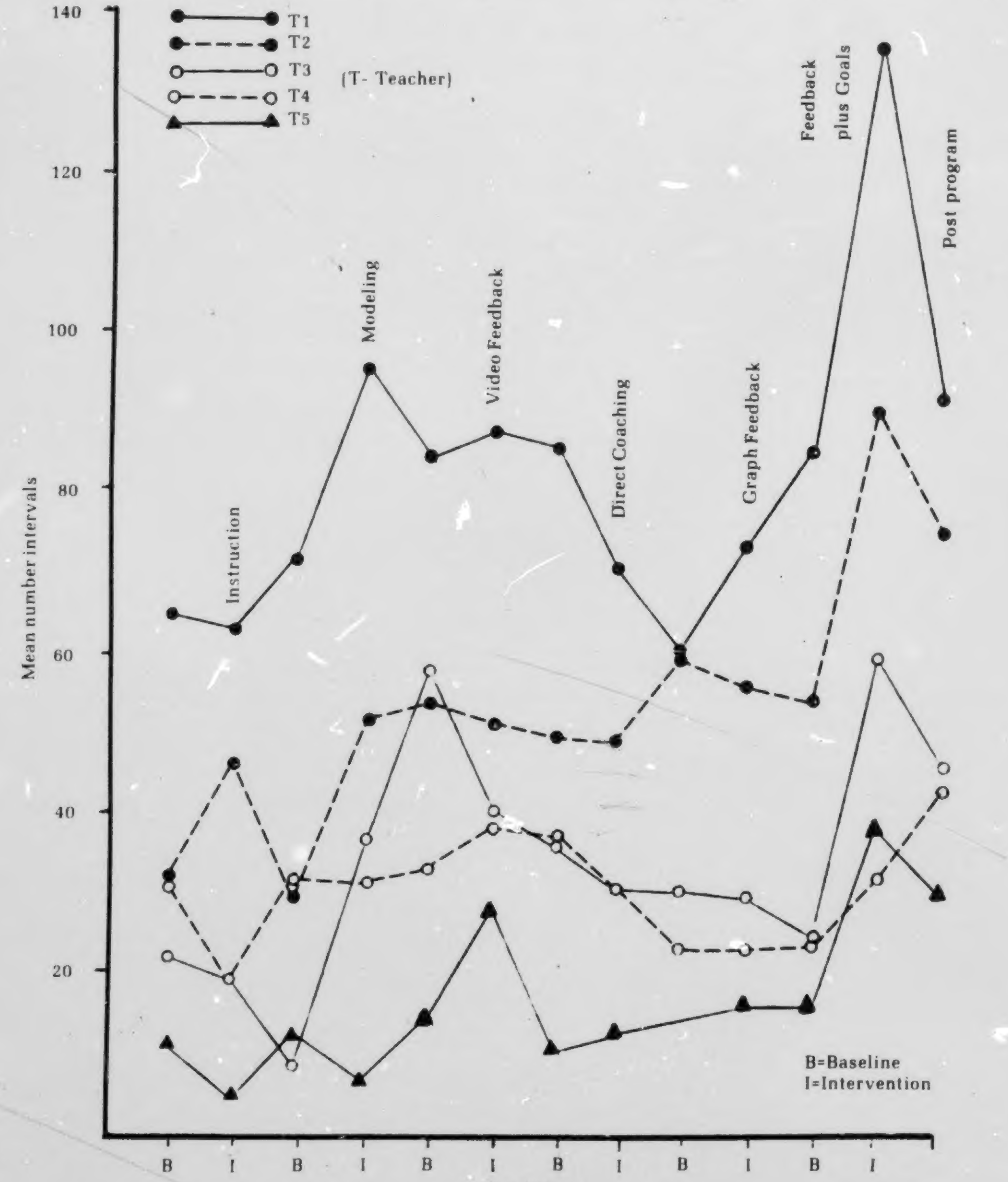

Figure 2
Mean number of intervals per 15 minutes in which teacher praise (academic plus management) occurred: Individual teacher performance. (Tanaka-Matsumi, 1977, p. 19)

An inspection of the individual teachers' baselines and performance in successive training conditions demonstrates that their responsiveness to different techniques varies widely, though some techniques, such as modeling or feedback with goal setting, increased most teachers' praise rates. Thus, an evaluation of the effectiveness of training calls for a careful analysis of the individual teacher's performance. While some techniques seem to demonstrate universal effectiveness under a variety of conditions, it is also important to recognize that the degree of effectiveness may differ considerably depending upon the trainee.

As a group, reliable change in teacher praise rate was effected especially by the second and third training conditions, that is, modeling and videotape feedback. The first training condition of didactic instruction did not result in actual behavior change. Another change was observed after the feedback plus goal setting condition was instituted. Giving specific performance criteria and following up with results of actual performance increased teacher praise rate more than any other condition.

The investigators of this program (Sloat et al., in press) appropriately called the training effect "incremental." Because the design of the study did not allow an assessment of individual training conditions, one should interpret the results carefully. In other words, because the training was composed of six successive training conditions, the behavior change after modeling and feedback with goal setting does not necessarily demonstrate effectiveness independent of other techniques.

This extensive training tested the feasibility of on-the-job teacher development. As a package program, this study provided a model whereby teachers can learn behavior management techniques in the field. Further, a 12-month follow-up evaluation of target behaviors of the five teachers indicated maintenance of training effect and a high on-task rate of students in classrooms (Tanaka-Matsumi, 1976, p.22). The importance of KEEP in-house training for educational specialists cannot be stressed too much; it is they who will be the real influence persons offering consultation service to the community.

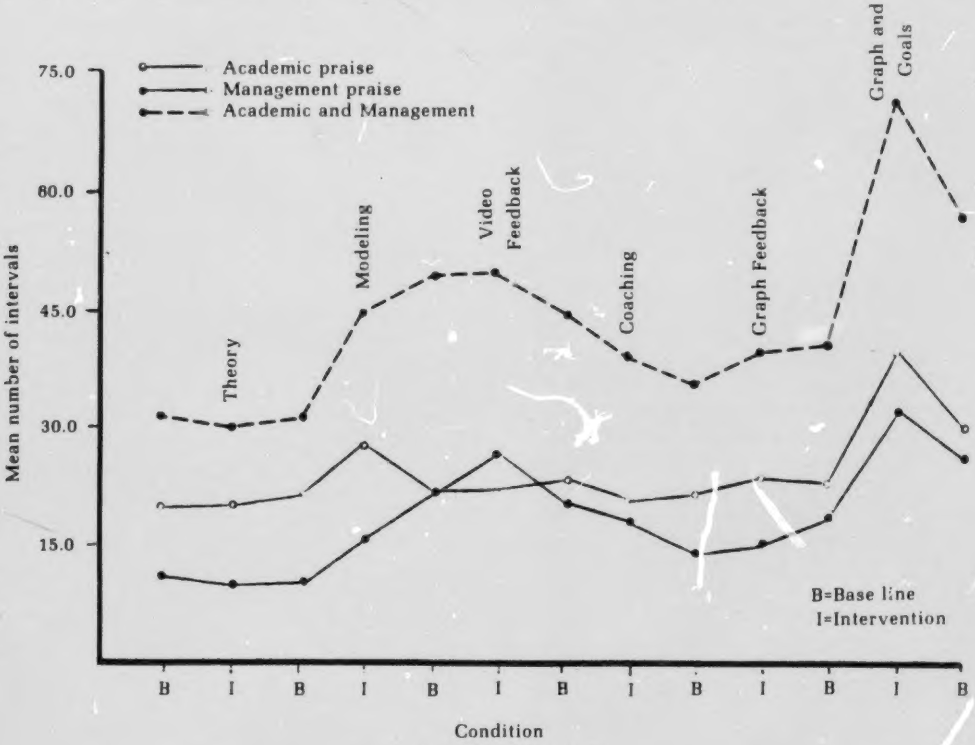

Figure 3
mean number of intervals in which academic management praise occurred per 15 minutes across conditions: Resident teacher training: Group performance (Sloat, Tharp & Gallimore, 1976)

KEEP Dissemination/Consultation Projects with Public School Teachers

In the triadic model the primary goal of the consultant is to change the behavior of the mediator; the consultant does not have direct contact with the target. In this program, the consultant is the KEEP educational specialist and the mediator is the DOE teacher. The target is the student in the mediator-teacher's own classroom. KEEP tested three patterns of consultation in the public school community: (1) Workshop; (2) University & credit course; and (3) Continuing resource consultation.

(1) The workshop model. This program is fully reported by Speidel and Tharp (1975; in press) who directed the workshop for two teams of three teachers who taught 56 children in kindergarten-first grade combined classes in two schools in the Honolulu community. The goal of the workshop was to increase the participants' frequency of contingent management and academic praise.

The strategy for disseminating teacher praise skills involved a sequential arrangement of five training conditions over three days at the KEEP demonstration school. These components were: (1) Didactic instruction on reinforcement principles; (2) Discrimination and observation training; (3) Guided practice in the classroom; (4) Videotape feedback; and (5) Group discussion.

The study was based on a pretest-posttest design with follow-up. Behavioral observation was carried out for both target and non-target reponses. All observations took place in the participant teachers' home classroom in the community.

The results indicated that academic praise increased from a group mean of 3.77 (pre-workshop) to 13.6 (post-workshop) per 15 minute interval with a follow-up rate of 12.0. A similar trend was observed for the management praise rate. Analysis of variance conducted for academic and management praises separately yielded a significant treatment effect (See Speidel & Tharp, in press for details). The treatment effect in this analysis is a combined effect of all five training conditions stated above.

Regarding the encouraging results, attention is called to two specific points: First, the workshop, as a package program consisting of five main training conditions applied to all six participants, produced a significant training effect in praise rate measured in their own classrooms. Second, an independent evaluation of the the five training methods was not made. Thus, the locus of treatment effect cannot be assessed from the overall results. However, a follow-up evaluation of the workshop demonstrated persistence of learned behavior in the teachers' own classroom. Given that teachers can be trained to increase contingent praise rate in their own classroom by participating in a three-day workshop, the workshop model as a means of disseminating skills-training appears promising and should be encouraged. One specific effort will be to develop an efficient package program using videotapes and texts that can be delivered to teachers in the school system.

(2) University credit course in behavior analyses for teachers. Teacher training at the university level provides an alternative means of teacher training (Becker, 1972; Hall, 1971), as seen in favorable outcomes reported for developing teacher skills (Hall, Fox, Willard, Goldsmith, Emerson, Owen, Davis, & Porcia, 1971; MacDonald & Gallimore, 1972). KEEP took one major action in cooperation with the University of Hawaii at Hilo (Johnson & Sloat, 1976), and trained 13 teachers from an elementary school located in a rural community who participated in a three-credit university course in special education entitled "Behavior modification for handicapped children." To pass the course, the teachers had to meet specific performance criteria in behavior analysis. The semester-long training course was divided into five components: (1) Didactic instruction for three weeks; (2) Extended didactic instruction for three weeks; (3) Guided practice for four weeks with one-day direct coaching at the KEEP Demonstration School; (4) Individual practice without supervision for four weeks; and (5) Feedback on teacher behavior in the classroom provided daily by trained observers for three weeks. In addition, trainee-teachers were given reading and practicum assignments throughout the course.

The observational data on teacher behavior in the classroom demonstrated steady change in praise rate occurring especially in the third and fifth conditions. Guided practice with coaching and feedback produced significant increases in praise rate while the early phase of didactic instruction did not produce any reliable change in teacher behavior. The results are consistent with the resident teacher training program regarding the effectiveness of feedback plus goal setting.

This study provided alternative means for training teachers via formal course procedures. Although, in this study, experienced teachers already in public schools served as student-teachers, this model is also applicable to prospective teachers. Instructing the prospective teachers in effective teacher skills before they obtain teaching assignments should be much more efficient than *post hoc* training.

(3) Continuing resource consultation. There is yet another model. KEEP conducted an extensive feasibility study of "continuing resource consultation" which aimed to offer consultation to a target school whenever demand for such service arose (Chun, Antill, & Tharp, 1975; Hao & Sloat, 1976). Underlying this model is a firm, continuing, consulting relationship with the consultee school. Using this helping relationship model, the KEEP consultant and the consultee-school teachers investigated a number of problem issues in the teachers' own classrooms. From the interaction of the consultant and the consultee, specific interventions were instituted. Thus, this project used a problem-solving strategy and dealt specifically with curricular-related problems rather than teacher behaviors.

The school consulted was located in a rural area on the Island of Hawaii. This elementary school had a high proportion of Hawaiian and part-Hawaiian students who manifested academic problems. The students had problems of motivation and academic underachievement when the consultation relationship was first established. The immediate consultative task surrounded the issue of upgrading students' academic performance, especially in mathematics. The problem-solving strategy involved: changing the physical arrangement of the classroom, forming homogeneous ability groups of students, and instituting an appropriate reward system for students. This intervention resulted in an increase of 100% in mathematics output as measured by the number of math pages correctly completed on an experimental test.

The second year of consultation involved improving reading skills of students in the same school, and KEEP introduced a curriculum which facilitated learning of sound symbol relationships. The results were positively evaluated by the school and students demonstrated improved reading as compared to students who were maintained under the old system. Both in reading and mathematics, KEEP was able to help Hookena teachers improve the performance of their students. The continued interest shown by those teachers supports the usefulness of this model as a continuing pattern of consultation over distance. This project produced excellent results in testing a long distance continuing consultation model.

The problem of distance and isolation will challenge any consultation strategy. Another project that KEEP conducted was in Hana, Maui, a small village which had only one school. The consultation team flew to this school and offered concentrated training to four elementary school teachers in contingent positive attention via guided practice (Sloat, Tanaka-Matsumi, Ah Ho, & Sueoka, 1977). One alternative to this direct consultation/training would be to train local-based consultants in consultee-schools, with KEEP acting as a headquarters for this service. In fact, a pilot study was conducted by training local volunteer observers in a rural district of Hawaii. These observers provided data for teachers in the classroom.

Program Comparison

Attempts will be made in this section to compare the different methods of consultation as each was reviewed, and to derive a measure for assessing relative effectiveness. To facilitate an inter-program comparison, the comparable data on the three programs (workshop, resident teacher training, and university course) are presented in a summary form in Table 1 for the criteria of "academic praise" only (see Tanaka-Matsumi, 1976, for further details on other target behaviors). (See next page for Table 1)

First change scores from pre- to post-training were computed; they were then transformed to percent change according to the following formula:

% change = (Postrate - Baseline rate)/ Baseline rate x 100. An examination of Table 1 reveals that all three programs resulted in significant change in teacher praise rate. The considerable individual variation in praise rate is noteworthy. The teachers in public schools, in general, had a lower praise rate than KEEP teachers. Under such varied individual baseline rates, the post-training data revealed differences in percent change which seem to reflect the original inter-individual baseline differences. Those who started with lower baselines achieved a much higher percent change in praise rate, and those who already had some level of skill showed a lower rate of increase. This method of comparison, then, is too much influenced by individual differences in trainees' initial level of performance.

Further, an examination of KEEP teachers' data reveals that they reach a performance ceiling at which point it is technically not possible to increase the rate, nor does such increase produce positive effects on the part of students. The combined effects of differential individual baselines and a necessary ceiling on praise rate lead to the conclusion that percent change is not a valid measure in teacher training programs.

Therefore, an assessment of the relative effectiveness of different patterns of consultation and training calls for a more reliable measure. To what extent does a high contingent praise rate

TABLE 1

Mean Frequency of Academic Praise per 15-Minutes
Across Three Different Programs

Subject	Baseline (pre)	Post	Pre-post Change score	% change
1. Workshop				
Teacher 1	1.67	8.00	6.33	379.0%
Teacher 2	3.67	8.50	4.83	131.6%
Teacher 3	7.00	14.80	7.80	111.4%
Teacher 4	2.00	22.80	20.80	1040.0%
Teacher 5	2.30	8.00	5.70	247.0%
Teacher 6	6.00	19.80	13.80	230.0%
Group	3.77	13.60	9.83	260.7%
2. Resident KEEP teacher training (X number of intervals)				
Teacher 1	35.64	41.04	5.40	15.1%
Teacher 2	16.74	48.68	31.86	190.0%
Teacher 3	19.44	30.24	10.80	55.5%
Teacher 4	21.60	15.48	-6.12	-28.0%
Teacher 5	11.16	22.68	11.52	103.2%
Group	20.91	31.61	10.69	51.7%
3. University course (X number of intervals)				
Teacher 1	3.96	13.68	9.12	245.0%
Teacher 2	5.94	8.46	2.52	42.4%
Teacher 3	3.42	5.04	1.62	47.3%
Teacher 4	5.40	11.52	6.12	113.3%
Teacher 5	3.78	15.66	11.88	314.2%
Teacher 6	5.58	20.34	11.30	202.0%
Teacher 7	3.78	15.30	11.52	304.7%
Teacher 8	1.98	9.90	7.92	400.0%
Teacher 9	2.52	11.16	8.64	342.8%
Teacher 10	3.78	14.22	10.44	276.1%
Teacher 11	3.78	11.70	7.92	209.5%
Teacher 12	10.98	18.72	7.74	70.5%
Teacher 13	0.72	6.48	5.76	800.0%
Group	4.27	12.47	7.88	184.0%

enhance student performance? Naturalistic observations of the rates of teacher approval and disapproval in the classroom indicate different rates according to grade level (White, 1975). Such an observation seems to suggest that teachers have baselines in accordance with the educational level of children. However, researchers interested in teacher training have not come up with any systematic assessment of a possible functional relationship between different teacher praise rates and student behavior. Questions regarding the functional relationship between varying degrees of praise frequency and student behavior are yet to be investigated. Most studies have concentrated only on the increase of contingent teacher praise. Teacher-training programs, in the future, must attend to this criterion issue.

The programs reviewed in this report indicate that there is a need to make careful analyses of the individual teacher performances after some basic skills are taught in groups. The studies also point out that an increase in teacher praise rate can be achieved in a relatively short time through workshops. The question regarding the choice of different patterns seems to be directed to another level of evaluation. The choice is not based upon which patterns yielded more change in praise, but rather upon the purpose of different patterns. If a brief period of dissemination and quick results are desired, workshops will respond to the need. Workshops can also be organized into packages and applied repeatedly with different groups of teachers. Also, such replications will yield more reliable data on this method. In adopting the workship method, however, attention must be given to the maintenance of learned skills in the teachers' own classroom. Unless one incorporates some generalization procedures into workshops, the effect of brief workshops may be short-lived.

University credit courses will benefit prospective teachers, as well as teachers who are already in public schools. This pattern of dissemination is especially important in effecting change in the overall conventional curriculum of the College of Education.

Thus, there are multiple utilities, depending upon the type of trainees and the requirements for training. Since each of KEEP's training/consultation patterns affected desired change in teacher behavior, a future study may be conducted on the application of those patterns to a wide range of teachers in the public schools. Another line of future study will be in systematically reducing the number of influence techniques applied per program. The question is: Is it possible to achieve equally effective results in lesser number of training conditions, and thus save time and resources? Across different programs, it is by now clear that didactic instruction *per se* did not lead to actual behavior change. It was also found that both specific performance criteria and appropriate feedback to the trainee are highly effective in increasing the target behavior. It is a research issue, then, if didactic instructions can be reduced or altogether eliminated. Similarly, research on varying the order of different influence techniques is necessary. Otherwise, KEEP dissemination/consultation projects cannot be reliably generalized to different settings, samples of teachers, or to various training variables including the trainer. External validity will be restricted.

External Validity of KEEP Dissemination/ Consultation Projects: A Methodological Concern

All the programs examined in KEEP's dissemination/consultation research produced positive results. It is necessary to attempt a summative evaluation of the entire dissemination/ consultation endeavor during the 1971-75 phase and then to advance plans for the future. One important question is to what extent one can generalize the findings beyond the immediate setting, teachers, or students. Campbell and Stanley (1966) and Campbell (1969) cautioned that quasi-experimental designs must be carefully evaluated against all possible alternative explanations, because the issue of control is not so clear as in true experimental designs. In the following, this paper will discuss three specific potential alternative explanations for the effects obtained in KEEP's dissemination/consultation programs. Such an examination from a methodological standpoint will aid in formulating firm plans for future research.

Reactive effects of experimental arrangements

A solid body of research evidence (Orne, 1962; Rosenthal and Rosnow, 1969) suggests that the mere fact of being in an experiment is in itself an unusual situation for many people, and that their reaction to being a subject or a trainee exposed to change techniques produces a rather unusual and special performance. Reactivity effects in the observation method are reported to interfere with the results of behavioral research (Johnson and Bolstad, 1973; Lipinski and Nelson, 1974; Romanczyk, Kent, Diamond, and O'Leary, 1973). The Hawthorne effect, as well as all other respondents' effects (Webb, Campbell, Sechrest, and Schwartz, 1966) must be taken into account.

Studies that continue over a length of time, such as the university course or the Hookena project, are more resistant to a subject's reactivity to the observation method than short programs. KEEP teachers are also observed every day and thus are habituated to frequent observations (Hagen, Craighead, and Paul, 1975). When the observation method is first introduced to a public school as a means of data collection and evaluationa, however, researchers must attend to both the observer and observee reactivity. Employing non-reactive measures (Webb et. al., 1966), along with carefully applied observation methods, will be likely to produce a triangulation of data which will be helpful in assessing the validity of the study in question.

Multiple treatment interference

This factor is especially relevant to the repeated measures design: where multiple treatments are jointly applied, effects atypical of the separate application of the treatments may be produced (Campbell, 1969). In teacher training, it is often the case that two or more behavior change techniques are applied to a single subject. Trainees may react differently to a repeated application of several techniques (e.g., instruction, modeling, and feedback) than if each technique was applied individually. The mere order of the application of different techniques may produce transfer effects from one condition to another.

Experimentally, the counter-balancing technique or a random arrangement of different treatment conditions is suggested (Underwood, 1957). This method, however, will necessitate a factorial design, where a large number of trainee-teachers are needed. This is not always possible in applied settings, nor is it always desirable in terms of expense and efficiency. KEEP opted for another measure: the order of presentation of techniques was based upon a logical analysis that made the most sense from the standpoint of how the techniques would be actually used in an operational program. As long as it is clear that the effects are cumulative or combined effects, and not independent effects of each of the influence techniques, this method offers a viable way to cope with this extraneous factor.

Interaction of subject selection and training (treatment)

An important factor that can limit the external validity of training research is an interaction between the selection process of trainees and the training conditions. This refers to the possibility that subjects selected for training represent a special group of teachers who respond to the treatment (training) in a special way that limits generality of findings to other groups of teachers. Since the KEEP dissemination/consultation projects used volunteer teachers from public schools, those teachers may constitute a different group of teachers from the larger DOE teacher population. Their motivation for improving teaching skills may be higher than others and they may be more "evaluation apprehensive"

(Rosenberg, 1969) than others. All these volunteer subject effects (Rosenthal and Rosnow, 1974) offer a challenge to the results. In the future, an investigation of participant-teacher variables may be necessary. Another way of coping with the problem of volunteer subject effect is conducting many replications of the same treatment (training) using different groups of teachers each time. In this way, by attempting to examine nonspecific effects, one can enhance the generality of influence techniques and patterns of training/consultation.

These considerations may restrict the generality of KEEP findings. However, methodological examination may also make KEEP's conclusions firm and avoid misapplication of strategies to be used in the community. At this stage every alternative hypothesis must be considered to reduce possible interference of extraneous variables with the main effects of training *per se*.

Generality of Methods

Are the methods studied thus far by KEEP indeed appropriate for the training of any teacher behavior, or are their effects specific to the goal of increasing teacher praise rates? If Tharp's (1975) assumption is correct, that means of influence are limited and ubiquitous, the workshop/training consultation formats and their various components should indeed be appropriate as KEEP increases the list of exportable findings. However, it remains an empirical question, and KEEP must attend carefully to needed alterations in the training programs as they focus on, for example, the teacher behaviors required to develop children's comprehension in reading.

Perspectives of KEEP's Dissemination/ Consultation Projects

The purpose of this section is to summarize the previous material and to advance suggestions for future studies. Over the past five years, KEEP has laid a foundation for research on dissemination/ consultation projects. The first five years, as reviewed, concentrated on the dissemination of behavior management techniques to DOE teachers, and, through empirical analyses of different patterns of dissemination/consultation, KEEP was able to generate a body of new findings indicating a strong possibility that public school teachers can be trained to use those principles in their own classrooms. The primary achievement of KEEP, viewed internally, lies in the development of a strong team of educational specialists trained to function as teachers, researchers, and consultants. The resident teacher-training program has contributed greatly to the development of behavioral analyses in those educational specialists. Their performance has kept a high on-task rate of KEEP students through social reinforcement techniques. Externally, KEEP demonstrated the strong feasibility of a variety of consultation patterns. Each of them produced positive results in teacher praise rate. One project, the Hookena project, researched curricular variables, and it tested effectively a model of consultation based on a continuing relationship.

The initial question of investigating different patterns of consultation in the community is already partly answered. An attempt was made to derive a measure with which to compare relative effectiveness of different patterns of dissemination/consultation. It was suggested that one research question in the future will be setting up appropriate performance criteria for trainee-teachers. Research in behavior change of teachers will enter a new phase that will deal with influence techniques in a wider perspective. Merely increasing teacher praise rate will not answer the ultimate need of teachers. In the future an increased emphasis will be placed on a combination of goals and individual data-based feedback.

Given positive results in the overall assessment of KEEP dissemination/consultation programs, the following three questions appear important for the further development of KEEP's consultation research.

1. KEEP-DOE relationship at the teacher's level

Foremost among the problems is the establishment of positive community-wide working relationships with DOE teachers. Although a formal agreement between the DOE and KEEP exists, it does not guarantee *actual* participation of teachers in KEEP projects. Thus, increasing DOE teachers' incentive to investigate new teacher skills becomes important. Public relations are now being critically examined by KEEP and a public relations office was established recently to cope with all issues concerned. It is expected, therefore, that KEEP will now attempt to build more effective relationships with the DOE teach community, especially in the Kalihi area, the center of KEEP activities. Establishing a network of communication in areas of major concern is highly desirable.

2. Design of studies

KEEP has demonstrated via the series of dissemination/consultation studies that a change in teacher behavior can result from training composed of a number of influence techniques. Emphasis has been placed on maximizing the effectiveness of behavior change methods by combining various techniques into one package program. However, it may be possible to reduce the number of training components and still attain the same purpose.

Systematic variation of the type and the number of influence techniques and a comparison of different outcomes of such studies may answer this question. Systematic replication will take into account how to reduce investment of time and resource. A cost-benefit analysis concept offers a viable way to evaluate KEEP dissemination/consultation research, and it is now under investigation. KEEP will face the question of finding ways to disseminate more with less cost; this will become an important research variable in the next operational phase of KEEP projects. Methodologically, therefore, KEEP should investigate further the possibility of systematically varying researchable aspects of designs. Also, continued examination of observation methods is necessary.

3. Local-based consultation

A relative cost-benefit evaluation of various KEEP projects in dissemination/consultation may, in the future, lead to the idea of increasing the economic efficiency of long-distance consultation. The Hookena and Hana projects provided a trial-and-error experiment in long-distance consultation. As suggested in the preceding section, the idea of local-based community school consultation deserves more attention given the goal of expanding the service delivery system of KEEP. Developing resident consultants will increase the range of KEEP's social impact beyond the current operational areas. It will also cut down on cost and time for KEEP staff, as well as provide consultants who know more about the school in question and its social environment. In other words, this idea is directly related to naturally available social resources in the community, and it leads to the question of expanding the triadic model of consultation to the areas where KEEP dissemination/consultation is desired.

KEEP's pilot study in developing resources for regular teachers has resulted in providing trained locally available paraprofessionals to serve public school teachers in their own classrooms, and they were received positively by those local teachers (Johnson & Sloat, 1976). The positive outcome of this project is, however, due largely to the presence of a field supervisor who incorporated the project into his university course.

It is not always possible to find appropriate field supervisors who can train local consultants. As models of consultation develop, utilizing intricate networks of social resources, it is important to ensure central supervision and coordination. For example, the Vermont Consulting Teacher Program (Egner and Lates, 1975) has an excellent network of local resources at various levels of state-wide administration and practice of teacher consultation. However, lack of inter-resource communication may produce problems that are not present in other less complex social models of consultation.

If the triadic model is to be extended into distant schools and districts, then we must carefully examine how much influence KEEP can maintain and develop without losing its impact upon the distant trainees. Under the proposed system of local-based consultation, it is still very important that KEEP act as headquarters to insure that the standard of consultation is high. Therefore, while advancing the idea of developing a system of local-based consultants utilizing increased social resource, the authors also call attention to potential problems which need to be investigated in the future.

The theoretical framework supported by the triadic model served as a powerful vehicle for KEEP dissemination/consultation projects. It has made possible the accumulation of a vast amount of information regarding the structure of dissemination/consultation research. It remains to be seen whether KEEP's involvement in the Hawaii school community will be accelerated in the coming year.

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CULTURE AND THE ROLE OF CLIENT EXPECTANCY

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I. Introduction

When Leonard Ullmann lectures on the sociology of behavioral science, he is fond of pointing out that one experimenter's error variance is another experimenter's independent variable. This truism is strikingly apparent when one considers the concept of "expectancy" in psychotherapy research. Since its "discovery" in the early 1950's (Rosenthal & Frank, 1956), this variable has assumed numerous roles across an assortment of theoretical and empirical publications. Some writers have touted expectancy as an active change-producing ingredient in psychotherapy (Chance, 1959; Fish, 1973; Frank, 1961; Goldstein, 1962; Lennard & Bernstein, 1960; Lick & Bootzin, 1975; Rosenthal & Frank, 1956). Others delegate expectancy to the domain of "non-specific treatment effects" and seek to control for it (Bednar, 1970; Fish, 1973; Lick & Bootzin, 1975; Paul, 1966; Thorne, 1952), while their colleagues conducting outcome studies find it a nuisance variable (confound) in assessing specific procedures (Thoresen & Mahoney, 1974) or use expectancy as a rival hypothesis to demolish evidence supporting specific techniques (Kazdin & Wilcoxon, 1976; Russell, 1974).

Although it has served many masters in the research literature, the concept of expectancy retains its potency as a vehicle for describing significant elements of the client-therapist relationship, for explaining certain process and outcome phenomena, and for linking the therapeutic encounter with facets of the sociocultural milieu in which it is embedded. This paper will explore the descriptive and explanatory utility of the construct as an avenue for understanding the relationship between the client's subjective culture and the social influence processes associated with psychotherapy. Subjective culture is defined as a culture group's characteristic way of perceiving its social environment. The perception of rules and the group's norms, roles, expectations and values are aspects of subjective culture (Triandis, 1972, p. 4).

Unfortunately, balanced against the usefulness of expectancy as a key explanatory process are a number of nagging limitations. The promise which this concept holds for integrating observations at the cultural, inter-personal, and individual levels, is somewhat undermined by conceptual and methodological short-comings. Furthermore, when expectancy is taken beyond the realm of explanation and used to modify client behavior, ethical considerations become important as well. Such restrictions are not unique in clinical research, nor are they insurmountable, yet they should be fully considered when providing a balanced examination of client expectancy in psychotherapy.

Three interwoven themes are presented in this paper along with a discussion of critical issues. The first theme examines the belief system which a client brings into the initial treatment contact, and the importance of taking this system into account as a prerequisite for the evolution of a common client-therapist conceptual framework of therapy. Second, a model of the expectancy process is offered suggesting how this variable, operating across therapeutic techniques as well as cultural boundaries, acquires a pervasive impact during the initial interviews. Third, the powerful social influence procedures employed by therapists to manipulate and shape client expectations are discussed. In a sense, these procedures represent tactics for washing out the disruptive influence of pre-therapy beliefs or using them advantageously to motivate and persuade client participation. Finally, the problems and limitations associated with this concept are reviewed, the focus being upon concerns of methodology and ethics.

II. Cultural Determinants of Role Expectations in Psychotherapy

A. Sociocultural Mechanisms in the Acquisition of Role Expectations

To advance a model accounting for the effects of expectancy in psychotherapy, features of the sociocultural context which give definition to the expectancy process must first be described. Obviously, the client's membership in a sociocultural group carries with it broad implications for his subsequent behavior vis-a-vis a therapeutic enterprise. Through socialization, culture members are recipients of specialized information regarding the nature of "psychological disorder," its supposed etiology, presumed cures, legitimate healing agents, and appropriate goals of treatment. This knowledge becomes encoded in an individual's belief system as a set of well-defined expectancies about role behaviors, techniques, and course of illness. These expectancies are subsequently evoked upon entering a treatment setting.

Anthropologists have stressed the importance of taking into consideration cultural beliefs about mental illness and its cure, particularly when health care is introduced into a community (Fabrega, 1972; Lieban, 1973). In fact, the field of medical anthropology was conceived in response to the failure of modern medicine to gain acceptance in non-Western settings. Students of culture are quick to point out that the pattern of beliefs surrounding mental illness is close to the core of a people's culture because it touches the very network of interpersonal relations that bind a community together (Cumming & Cumming, 1955). These beliefs, frequently anchored in other parts of culture like religion and social values, provide a framework for interpreting symptoms and guide social action in response to them. Predictably, the accepted explanations about the causes of disorder determine what people will do to cure them, the locus of intervention, and how they will behave toward the sick person. In some cultures sickness is seen as punishment for wrong doing (Hallowell, 1963), while in others it represents a sign of discord in a social group (Lieban, 1973). In the latter case, the doctor works to bring to light and expose to ritual treatment the tensions and aggressions in the group's interrelationship (Turner, 1964). Denko (1966) carried out a cross-cultural survey of how preliterate peoples explain disturbed behavior. The tripartite typology he derived is surprisingly modern; it includes forces from the past (e.g., prenatal maternal carelessness), the present (e.g., possession, noxious environment, sorcery, punishment for guilt), and the unfortunate's personality.

In summary, the client's culture is an active participant in any therapy. At the interpersonal level, it defines the referral process, prior social labeling, the saliency of the dysfunctional fit with social group, and the potential range of acceptable explanations. At the level of cognition, culture provides an anticipatory mapping of the healing situation. The client expects to encounter a healer enacting a proscribed role and in turn to assume the proper patient role. Therapist qualities, tactics, pronouncements, and prescriptions are expected to operate within a consensual world view. The realization of this culturally determined set of expectations during initial patient-healer contact is assumed to have profound implications for the subsequent course of therapy. Congruent expectations lead to the arousal of faith in the treatment and positive prognostic expectancies. This belief serves to combat demoralization while heightening the client's hope of relief (Frank, 1973). Torrey (1972) and others (e.g., Coe & Buckner, 1975; Frank, 1973; Gallimore & Turner, 1969) suggest that faith reaches its peak when: (1) there is a high degree of agreement between the therapist and client in naming the disease and its cause; (2) when both share the strong belief that the techniques used are of value in helping; and, (3) the therapist's personal attributes match the client's expectations of what a therapist should be like. The following section reviews specific cultural and subcultural therapeutic expectations.

B. Expectations Across Cultures

The counseling and psychotherapy research literature is remarkably silent on the issue of cultural variation in treatment expectancies. In contrast, within-culture differences are well reported, as writers have long recognized the relationship between social class, behavior in therapy and outcome (e.g., Hollingshead & Redlich, 1958). Other data sources do converge, however, to offer information on this topic. These sources include ethnographic accounts, studies of international students in the United States, and recent work in providing mental health services for minority groups. A sample of this work will be provided to underscore the contention that pre-therapy expectations are dramatically colored by cultural antecedents and merit due consideration.

Clark's (1959) early ethnography of a Mexican-American community in California found explicit expectations about how a medical practitioner should behave. The curer's role was to mobilize the resources of the family group and friends. The practitioner enacting a scientific and authoritative medical role was in conflict with the family as authority and its preference for a personal, unhurried style of medical intervention. Interestingly, Acosta and Sheehan (1976) found that 20 years later at least one sample of Mexican-American college students has a high regard for Anglo professionals than for those identified as coming from their own ethnic group. This finding is in surprising contrast with an earlier group of studies examining client and therapist race and treatment processes (Carkhuff & Pierce, 1967; Wolkon, Moriwaki & Williams, 1973; Yamamoto, James, Bloombaum & Hattem, 1967). These authors report that blacks prefer black therapists, show more change when matched with same race therapists and tend to drop out early or do poorly with certain white professionals.

Medical anthropologists working outside of the United States have also observed the actions of cultural expectations in healing situations. Marriott (1955) found that traditional healers in Northern India were masters at satisfying the expectations of the villagers. Confidence, trust and interpersonal attraction were enhanced by rendering culturally expected devices like magical medicines and performances within the family context. The healer operated effectively among the people, based on his reputation for superior knowledge of the body, the awareness that his motivation was based on piety and the desire to enhance religious merit, and because he took full responsibility for making and guaranteeing the medicines used. A Western trained doctor diminished his effectiveness by seeking to establish an egalitarian relationship with village patients, thus reducing the security they sought in a dependency role, not including the family and failing to make and guarantee his own medicines. Working in Rajasthan, India, Carstairs (1955) also noted that what is expected of a healer is reassurance. The healer, serving as a link between mortal man and the purposeful cosmos, reassures the group that the treatment will work unerringly. When both the healer and the patient have faith in the cure, it will work. It is deemed "malpractice" by villagers for a physician to suggest that his medicines may not always have the desired effects.

The reactions of international and ethnic minority students to counseling services at American universities provide a rich source of information bearing on the relationship between culture and psychotherapy. Arkoff, Thaver and Elkind (1966) and Tan (1967) surveyed the counseling ideas of Asian and American students. Conforming with the assumption that the Asian social structure is characteristically authoritarian, these authors found a view of therapy among Asians marked by its authoritarian orientation—directiveness, submission and nurturance. The therapist is assumed to be an experienced "teacher" who gives information and advice on a specific course of action. These students positively endorsed such statements as "The good therapist is like a father to his patients." Moreover, Asians sampled did not expect outside assistance for solving internal (psychological) conflict. Rather, internal adjustment should be worked out by the individual himself; mental health is thought to be enhanced through exercising will power, avoiding unpleasant thoughts, and occupying oneself with pleasant thoughts (Arkoff, et al., 1966). Caudill's (1959) studies of Japanese psychotherapy underscore the belief in this strategy. The focus of therapy is away from psychodynamics and feeling exploration and instead rests upon disengaging oneself from surrounding problems and occupation with constructive, concrete pursuits. (See Kondo, 1976, Murase, 1976, Tanaka-Matsumi (1976) for details of Japanese indigenous therapies.)

Patterns of clinic use by Asian-American and foreign students shed further light on expectations associated with psychological treatment. Psychiatric facilities on U.S. campuses are steadfastly avoided while educational counseling centers are overused in comparison with native students. Denny (unpublished manuscript) found that 11% of registered foreign students yearly visit the University of Hawaii Counseling and Testing

Center, while Sue and Kirk (1975) reported that nearly 50% of the Chinese-American women had made use of the Berkeley Counseling Center. On the other hand, these students will come to campus psychiatric facilities only as a desperate last resort; many being sent by friends, relatives and neighbors involuntarily (Klein, et al., 1971). Reluctance to be seen in psychotherapy possibly derives from Asian students' difficulty in labeling themselves as having emotional problems and the characteristic somatic expression of their complaints (Marsella, Kinzie & Gordon, 1973). Emotional problems are taken to health services where many visits are made for multiple psychosomatic and anxiety based complaints. Moreover, in the case of Chinese-Americans, if some problem does arise much effort within the family is expended toward hiding it from public view. Similarly, there is a stigma attached to being in need of psychiatric services; it is associated with shame, loss of status and the fear of being sent home as a failure. Unfamiliarity with such services, since they are not found back home, and uncertainty as to what to expect from them or the expectation that they will force unwanted changes are other factors. From the perspective of the foreign student, the viable sources of help for their personal problems are first, self-reliance (Arkoff, et al., 1966; Chunnual, 1971), then fellow countrymen, followed by faculty advisors, educational counselors and medical health personnel (Pedersen, 1975; Sundberg, 1976). These alternatives do not carry the stigma of a psychiatric intervention and follow more closely the student's conceptualization of what his problems are—academic and/or physiological.

Summarizing their experiences with Chinese students, Sue and Sue (1972) offer specific guidelines for counseling practices that are sensitive to client expectancies. These suggestions may be generalizable to other Asian groups as well. At the onset, the counselor should remain low key and non-confrontive to avoid increasing the client's shame. He should be prepared to respond to the "superficial" problems that client brings up while remaining aware that discussion of educational difficulties and physical symptomatology serve as channels for dealing with "real" problems. Therapy should be structured to reduce ambiguity and demands for openness and expressiveness which make the Chinese client extremely uncomfortable. It should be pragmatic, with the therapist providing suggestions geared to immediate practical application. Self-disclosure and client relaxation in the sessions are enhanced by assurances of complete confidentiality and the communication of concrete guidelines of what to expect. Furthermore, encounter group formats with self-disclosure to strangers are definitely contra indicated with these students (Alexander, et al., 1976).

C. Expectations Across Social Class Boundaries

The pattern of partial and scattered findings relating client culture with treatment expectancies is surprising given the strong multi-ethnic flavor of American society. This shortcoming has not carried over to the consideration of different expectations based on level of social class. Researchers and practitioners have long recognized that socio-economic status predicts behavior in therapy of both client and therapist and numerous writers have sought to map this relationship (e.g., Aronson & Overall, 1966; Baekeland & Lundwall, 1975; Brill & Storrow, 1960; Frank, 1973; Garfield, 1971; Hollingshead & Redlich, 1958; Meltzoff & Kornreich, 1970; Overall & Aronson, 1963).

Early studies found consistent differences between lower-class patients and their middle- and upper-class counterparts in understanding the aims and procedures of therapy (Auld & Myers, 1954; Hollingshead & Redlich, 1958; Myers & Roberts, 1959; Overall & Aronson, 1963; Schaffer & Myers, 1954). These differences, both of degree and kind, may have eroded somewhat in the last 20 years with the widening availability of mental health services and efforts toward public education (Lorion, 1974). Nevertheless, a survey of class-linked conceptions of therapy is illuminating, especially given that therapists' attitudes toward and their selection of clients are frequently predicted on social class (Garfield, 1971; Meltzoff & Kornreich, 1970), and that low-income status relates to low acceptance, organic type treatment and high attrition rates in individual therapy (Baekeland & Lundwall, 1975; Hollingshead & Redlich, 1958; Kahn, Pollack & Fink, 1957; Lorion, 1973, 1974; Sue, 1976). Early observations were offered by Myers and his colleagues (Auld & Myers, 1954; Myers & Roberts, 1959; Schaffer & Myers, 1954). They noted that lower-class patients saw the therapist as a magician, nerve doctor or mind reader, and assumed that medical treatment was necessary for their organic-induced illness. Moreover, such patients were often affronted by the notion that their difficulties had an emotional base.

Extensive sampling by Hollingshead & Redlich (1958) as well as documentation from numerous psychometric and clinical-observational sources (Aronson & Overall, 1966; Bernstein, 1964; Frank, 1973; Overall & Aronson, 1963; Riessman, Cohen & Pearl, 1964; Yamamoto & Goin, 1965) provide an integrated picture of the lower-class patients' set of expectations. The therapist is expected to take an active medical role, treat physical symptoms with pills and needles, be fast in solving the problem, authoritative, impersonal, and not expect verbal exchange or insight as central to the treatment. The client sees his role as a passive recipient of treatment administration. He considers himself an object rather than a vehicle for change and does not believe that introspection, self-criticism, or self-initiated behavior change are suitable means for regaining health. Szasz and Hollender (1956) have termed these expectations the "guidance co-operative" model of therapy as opposed to the "mutual participation" approach which the North American therapist generally desires.

Although Garfield (1971) has suggested that all social groups share misconceptions about treatment equally, and therapists simply overemphasize inappropriate expectations in low class applicants, it seems safe to assert that upper- and middle-class clientele anticipate with greater accuracy the experience encountered in therapy. From education, association with professionals, and prior experience by family and friends, they are simply in a position to possess veridical information. Indeed, Howard and Orlinsky (1972), taking a socio-cultural view of current psychotherapy, comment that "the major *de facto* function of our psychotherapy activity is as a kind of higher education in the development of interpersonal skills and emotional capacities" for middle- and upper-class individuals with interpersonally demanding and taxing emotional roles to play in society (p. 658). The general congruity between the middle-class client and the middle-class world view of most therapy systems and therapists is evident and discussed in abundance elsewhere (Frank, 1973; Hollingshead & Redlich, 1958; Howard & Orlinsky, 1972; Kessel & McBrearty, 1967; Kinzie, 1972; Pande, 1968; Patterson, 1958).

D. Expectations in General

Ignoring class and cultural variables, what can be said about applicants' treatment expectations in general? A thorough investigation of this domain, one which would encompass the breadth of potential expression of expectancy, has yet to be done even for homogeneous populations. The questionnaires and self-report instruments which have been developed tend to employ a constricted sample of theoretically biased items (e.g., Apfelbaum, 1958; Begley & Lieberman, 1970; Overall & Aronson, 1963). Unanswered is the question of what are the spontaneous notions that clients initially bring to treatment concerning significant aspects of the situation: the physical setting, duration of treatment, therapists' personal attributes—sex, age, training, ethnicity, mannerisms—techniques employed, adjustment goals, problem classification, explanation of causality, who else will be involved, social distance, the client's obligations and responsibilities, cost, and so forth. Open-ended interviewing of client and non-client samples would seem necessary to provide important, as yet ignored, clues and dimensions for further research.

The studies which do exist, however, provide glimpses of possible answers to this question. Garfield (1971) noted that clients, regardless of socio-economic status (SES), expect psychotherapy to require 5 to 10 sessions only for completion. A sample of Texan college students, containing members of all SES groups, indicated a preference for older male therapists, except for females with personal problems who preferred older women therapists. Not surprising, the degree to which clients felt that the therapist would "understand" their problems played the greatest role in determining the degree to which the therapist was preferred. Theoretically derived patient "sets" have also been identified with structured instruments. Apfelbaum (1958) used a Q-sort analysis to divide patient's expectations of their prospective therapists into three clusters: nurturant (protective), model (personally well-adjusted) and critical (probing). Overall and Aronson (1963) following the work of Hollingshead and Redlich (1958) divised a 39-item questionnaire tapping the four orientations of medical, directive, supportive, passive and psychological. Half of the items discriminated between lower- and middle-class patients (Aronson & Overall, 1966). Other dimensions thought to be critical to client expectations include Task versus Personal (the degree to which task-related or personal characteristics of therapists are selected as being of greater importance; Johnson, 1970), A versus B therapists (active and experiential manner versus passive or didactic in conduct of therapy; Berzins, Friedman & Seidman, 1969), and the tripartite AID measurement (Analytic; Impersonal versus personal affective approaches to the patient; and Directive, active techniques; McNair & Lorr, 1964; Begley & Lieberman, 1970).

In short, there appears to be scattered yet consistent empirical support for the contention that clients approach therapy with well-defined and individually diverse role expectations, therapist preferences, forms of support anticipated, types of advice sought, or type of medical care desired.

These cognitive sets vary in applicants as a function of their cultural, ethnic and socioeconomic backgrounds, although education and mass media may be homogenizing peoples' basic understanding of clinical intervention in the United States. The assessment of client expectations remains at a primitive level conceptually and methodologically. Adequate devices for appraising the full spectrum of client populations' or culture groups' expectations remain to be developed and applied. Yet, after establishing the contention that such cognitive sets exist and vary across identified populations, of what significance is this finding? The importance of this review becomes evident in the following section where it is proposed that the evolution of a working relationship in psychotherapy cannot mature, nor the development of hope, faith and compliance, without an adequate accounting of the patient's pre-treatment expectancies.

III. Expectancy Process in the Evolution of a Positive Working Relationship in Psychotherapy: A Preliminary Model

A. The Effects of "Met" versus "Unmet" Expectations

What emerges from a careful analysis of client role expectancies in the initial phase of treatment is a complicated picture of interaction between these beliefs, the therapist's preliminary behavior, and subsequent cognitive, emotional and behavioral responses on the part of the client. This process constitutes the developmental antecedents leading to the therapeutic alliance. A patient-professional alliance or working relationship is deemed critical by all treatment orientations. There are, however, approaches where the patient is treated through a "significant other," in which case the focus of the therapeutic relationship shifts appropriately (e.g., Tharp & Wetzel, 1969). Understandably, the attainment of a positive working relationship is a crucial milestone without which specific intervention efforts remain useless. However, once secured, this stage of therapy provides the framework within which an assortment of social influence procedures directed at client change can be introduced.

At this juncture, a scenario can be constructed characterizing events prior to the establishment of a working relationship. First, the applicant enters the treatment setting with his unique set of beliefs about what will happen and a conceptualization of what constitutes the problem. Secondly, he encounters an establishment pattern of behavior that represents the interviewing style, orientation, and system of personal attributes of the therapist, embedded in the micro-culture of a clinic, hospital or private office. At this moment, the client-therapist interaction makes salient one of two divergent and critical perceptions: (1) There is an essential congruity between expectations and what is encountered; or, (2) There is incongruity and discord between these expectations and the situation at hand. Both sets of perceptions have profound implications for subsequent client performance in and out of therapy.

Congruent role expectations mobilized the client's affective arousal in a positive vein, facilitating certain adaptive cognitions and hope-engendering self-statements. That is, the client recognizes that he is receiving the necessary and correct help he sought. It is coming from a recognizable expert possessing the required skill to ameliorate suffering, solve personal problems, and so forth. Under these conditions, positive prognostic expectations are evoked. The client has hope, confidence and faith in improvement; demoralization and hopelessness are counteracted. He is motivated to stay in treatment. The behavioral effect is to open the client to further social influence and induce a rigorous attending to therapist instructions. There is increased compliance in carrying out treatment procedures, while reality testing may be enhanced with resultant fear reduction and increased self-reinforcement. Further, the expectation of ultimate success serves to bridge the gap between the immediate, taxing tasks required by the therapist and the eventual improvement.

Essentially, congruent role expectations provide for the rapid, almost spontaneous evolution of a common client-therapist conceptualization of the problem. Meichenbaum (1975; in press) asserts that reaching an agreement on how to view the client's problem becomes a major therapeutic event, given that a particular conceptualization leads to specific behavioral procedures and subsequent behavior change that can be transferred to real-life situations. Moreover, to the extent that a reconceptualization provides the client with a new perspective, meaning, and understanding of his problem, it engenders a sense of control and hope which facilitate change as well. The Rumpelstiltskin effect (Torrey, 1972) of naming and explaining a problem in this fashion may by itself produce client improvement, a phenomena termed "assessment therapy" (Davison, 1969;

Meichenbaum, in press; Schachter, 1966; Wilson & Evans, 1976).

Sharply contrasting with this scenario is an alternative one arising from unmet expectations or incongruity between preconceived ideas and the actual clinical encounter. It is likely that a negative patient reaction (psychological reactance, resistance, countercontrol) will ensue when it is found that what is supposed to happen in order to receive help doesn't occur. Unless the initial contact is devoted to the skillful application of a "role induction" procedure (e.g., Orne & Wender, 1968) aimed at offsetting this condition, the likelihood of developing a therapeutic relationship is low and the probability of a "premature" termination or unsatisfactory outcome is high. The logic of this prediction is relatively straightforward. Unfulfilled expectations are sparked by a treatment rationale with low credibility (Borkovec & Nau, 1972; Kazdin & Wilcoxon, 1976), the absence of face validity in naming and explaining the problem (Meichenbaum, in press), perceived lack of relevance in the content of the therapist's responses (Rosen, 1972), and a therapist style, attitude, and manner which are unfavorably evaluated. Since these are associated with incompetent help, faith, hope, trust, and positive prognostic expectancies are not aroused. The client's internal dialogue under these conditions would be, "This therapist is a schlep; he doesn't understand me or my problem; he can't be of any use." Compliance with therapeutic instructions is minimal as the client fails to understand how demands made on him are profitable in reaching personal goals for change and relief (Heitler, 1976).

In short, discrepant expectations foster a failure in mutual collaboration; the patient-professional alliance does not materialize, social influence cannot be exercised, and further treatment contact is deemed useless and inappropriate. It should be recognized that these perceptions, discussed from the client's frame of reference, are equally applicable to the therapist's perspective. It is an interaction of the two participants' sets of expectations and evaluations of the relationship which ultimately determine the course and outcome of therapy. As Ullmann and Krasner (1969) point out, a therapist evaluating the client as a poor risk is less likely to establish rapport with him thus providing the conditions for a self-fulfilling prophecy.

B. Model of Expectancy Process

The two scenarios formulated above represent a distillation of the empirical findings in this area (c.f., Chance, 1959; Garfield, 1971; Hoehn- Saric, et al., 1964; Levitt, 1966). They may not represent the typical clinical case, but do serve to illustrate the general expectancy process as it occurs in varying intensities depending on the backgrounds of the two participants. However, the general expectancy process itself, and its component parts, remain to be further clarified, articulated and summarized. The concept is in danger of becoming ambiguous and haphazardly applied; it should be clearly distinguished from similar concepts such as preference, hope, and anticipation. In this section, a concise statement of what expectancy is and is not will be presented.

The process of expectancy can be analyzed into three components. It initiates with a cognitive event: A set of self-statements, organized as a belief system, is activated by the treatment encounter (Cartwright & Cartwright, 1958). These self-statements are particular anticipations of and assumptions about future events. They are probability statements held by the individual that certain consequences (reinforcements; desired/undesired outcomes) will occur as a function of specific behaviors in a given situation (Rotter, 1954). If I do X, Y will happen, and I will feel good/bad (Triandis, 1972). In fact, much role appropriate behavior is enacted in given situations based on the perceived likelihood that it will accrue desired consequences (Ullmann & Krasner, 1969). Translated into treatment relevant terms, within American society, the anticipation can take several forms: "If I go to the doctor, he will give me anti-depressants to make me feel better; Only professionals who can relate to the . . . (Black, Indian, poverty, WASP, Jewish, feminist) . . . experience are capable of understanding my problem; I've got to find a rational-emotive therapist, they're the only kind who really know what to do with these awful beliefs of mine; etc." Or, as might be found within another culture, Malaya, "The *bomoh* has direct contact with the *hantu* 'malevolent spirits,' our clan will pay him to propitiate the spirits and cure the women of *latah*" (Chen, 1970).

The second component of the expectancy process is the emotional arousal tied to the belief system. Goldstein (1962) terms "prognostic expectancies" those briefs regarding the anticipated gains accrued as a result of participating in psychotherapy. Therapy congruent with role expectancies heightens prognostic expectancies and hence the client's faith and confidence in a positive outcome. Research indicates that people will perform in such a way as to conform with their expectations (Brehm, 1976; Carlsmith & Aronson, 1963). This finding holds true for both members of the therapeutic dyad (Frank, 1973). Furthermore, the arousal of hope with the belief of treatment efficacy mobilizes associated feelings of optimism, energy, and well-being which in themselves may actually promote healing, especially in affective disorders (Frank, 1973). Positive emotional states counteract feelings of demoralization and hopelessness; they have a tonic effect on the client's physical health and motivation to try new behaviors (Murray & Jacobson, 1971). The elicitation of positive affect in this fashion with often profound and pervasive consequences on the client is the specific "non-specific" or placebo effect that emerges during the early phase of therapy (Rosenthal & Frank, 1956). Goldstein's (1960a) analysis of this phenomenon led him to suggest it as the active agent in "(un)spontaneous remission."

The third component of this mediational process is behavioral in nature. It involves selective attention and response utilization on the part of the client (Mahoney, 1974.) Once credibility and faith are established (congruent role expectancy), they motivate the client to rigorously pay attention to and comply with therapeutic instructions (Lick & Bootzin, 1975). The professional's personal influence is maximized; he gains potential as a model, teacher, discriminative stimulus, reinforcer, and so forth, introducing procedures that will result in permanent change. Lick and Bootzin (1975) posit that this process is an active change mediator when expectancy is manipulated in systematic desensitization. With high expectancy, there is an increased tendency to test reality after treatment with resultant fear extinction and self-reinforcement. Also, certain cognitive events (e.g., re-labeling of a feared object) controlling fear responding may be modified as well. Continuing in a learning theory vein, G.M. Rosen (1974) adds that the expectancy helps the subject relax, thereby facilitating non-reinforced exposure to the aversive stimulus.

To summarize, expectancy is a complex process involving the interaction of client beliefs with specific elements in the therapeutic situation. Clinical contact activates a set of "cause-effect" beliefs about appropriate and efficacious treatment conditions. The fulfillment of these necessary and sufficient conditions as construed by the client stimulates a positive prognostic prediction—"I will get well." Apprehending this, the individual is emotionally relieved, hopeful and optimistic. These positive emotions, acting alone, may be the basis for placebo-like effects. Finally, compliant, attentive, and treatment-appropriate behaviors are guided by the fulfilled expectancies. Client influencibility and openness to change are achieved. It should be clear now how expectancy is distinguished from anticipation or preference. Anticipation is what a client thinks will happen in therapy; preference is what a client would like to see happen in therapy; expectation is what the client believes to be the necessary and sufficient conditions for receiving adequate help which will lead predictably to a successful outcome.

IV. Manipulation of Client Expectancies: Tactics and Procedures for Overriding Initial Beliefs

The assumptions put forth in this explanation of the expectancy process are based on a review of existing empirical studies in this area. Over 100 publications were found dealing with some aspect of expectancy, either in the client, therapist or both, and several prominent reviews of this material are available (e.g., Goldstein, 1962; Howard & Orlinsky, 1972; Meltzoff & Kornreich, 1970; Luborsky, et al., 1971). Problematic to this literature is the conceptual confusion among the separate components of the process as distinguished above. Clouding of the concept by writers makes it difficult to pinpoint functional relationships between its individual components (for example, heightened expectancy of success versus the fulfillment of role expectancy) and variables associated with treatment performance and outcome. Similarly, other methodological issues impinge on the discussion of this process, especially problems of conceptual validation and independent measurement. These are taken up in the final section. Nonetheless, in the last 15 years, investigators have been busy manipulating what they presumed to be expectancy and looking for variation in subjects' performance in and out of therapy. The following section will review some of these efforts, looking at the tactics and procedures employed to enhance the client's responsiveness to the treatment situation. In essence, such studies have sought to either capitalize on knowledge of individual expectations and hence determine performance, or wash out individual differences with socializing instructions, social persuasion, or other social influence devices.

A. Role Induction Procedures

Once the preliminary studies by Goldstein and others (Frank, 1961; Frank, et al., 1959; Friedman, 1963; Goldstein, 1960b, 1962, 1966; Goldstein & Dean, 1966; Goldstein & Shipman, 1961; Lennard & Bernstein, 1960; Lipkin, 1954) began to consistently find that client expectancies of success were related to initial symptom reduction, and subsequent improvement at outcome, then investigators began to turn their attention to devising procedures for insuring expectancy arousal and shaping its content during the preliminary phases of therapy. At the Henry Phipps Clinic of the Johns Hopkins Hospital, an active group of researching clinicians quickly put forth and tested a model interview method (based on Orne and Wender's (1968) Anticipatory Socialization Interview) for inducing clients into appropriate therapy roles (Hoehn-Saric, et al., 1964). The concept of a role induction interview was not original, however. Rotter (1954) had earlier commented on socialization in therapy, advising that the therapist's expectations should be stated explicitly to the client and be the periodic focus of mutual attention. He further urged that the purposes, plans and goals of therapy, the respective roles and responsibilities of both participants, and their attitudes toward psychotherapy should be discussed at any time over the course of treatment. Similarly, Kelly (1955) noted about the same time that the client's conceptual framework of the therapy is a central issue, recognizing that it determines the role he expects to play and the role he expects the therapist to play. Ford and Urban (1963) added the observation that the patient's view is but one of three ways of analyzing and construing what is happening in therapy. In choosing a conceptual framework, it is possible to also adopt either a "theoretical" set of concepts, or make an eclectic compromise: using some of the client's concepts, some of the therapist's, and those of a variety of theoretical systems. Regardless of which concepts are decided upon, they determine the important events in therapy: what is listened to, what constitutes progress, and how different treatment phenomena are related to one another. This line of reasoning has culminated in Meichenbaum's (1975; 1976; in press) careful attention to the initial translation process in therapy, where there is special concern with how the therapist prepares the client to accept (implicitly) a particular therapy rationale with its accompanying treatment intervention. It is suggested that the client and therapist evolve together a common conceptualization so that the client feels he is an active participant and contributor to the process (Meichenbaum, 1976, p. 299).

The role induction procedure itself, as defined by the Phipps group, seeks to increase treatment duration and positive outcome through decreasing the incongruity between the expectations that a patient brings to the clinic and what actually occurs, and by raising hopefulness of a cure. With this rationale, an induction interview has developed composed of four separate components. Included are: (1) a general exposition of psychotherapy; (2) delineation and explanation of expected behavior of therapist and client; (3) forewarning of typical phenomena that arise in the course of treatment, like "resistance;" and, (4) induction of a realistic expectation for improvement within four months. Hoehn-Saric, et al. (1964) tested the induction interview on psychologically unsophisticated patients, finding that it improved their attendance rate, their performance in therapy as evaluated by their doctors, and resulted in increased ratings by independent observers on a measure of social effectiveness. Sloane, Cristol, Pepernik and Staples (1970) replicated the Hoehn-Saric, et al. study while separating the effects of socialization *per se* from the "faith" or prognostic expectancy induction. They found that socialization for role alone contributed to improvement at outcome. A host of writers following these intital efforts verified the efficacy of anticipatory socialization through initial intake procedures, pre-training, modeling, structuring referral information, film induction, and so on (Goldfried & Davison, 1976; Greenberg, 1969; Heitler, 1973, 1976; Jacobs, et al., 1972; Krause, Fitzsimmons & Wolf, 1969; Strupp & Bloxom, 1973; Truax & Carkhuff, 1967; Yalom, et al., 1967). However, as Heitler (1976) has pointed out, these studies can be criticized methodologically for not controlling for SES or prior sophistication regarding psychotherapy.

In conclusion, the anticipatory socialization or induction approach attempts to wash out or override previous beliefs discrepant from the system of behavior change about to be offered. The therapist attempts to shape client acceptance of explicit treatment parameters, inducing "good client" behaviors, harmonious conceptualization of the process and problem, and setting up awareness of the events to be recognized as "appropriate help" and indices of progress. The approach aims to impart "accurate" information, dispelling misconceptions and prejudice, that leads to enchancement of the client's motivation for treatment, an increase in his susceptibility to influence, and ultimately results in the therapist's positive perception of the client's suitability (attractiveness) for therapy. The role induction also permits the creation of a cognitive structure enabling the individual to correctly anticipate and organize his experience. The acquisition of this structure facilitates learning and retention of new and more elaborate sequences of behavior demanded at later stages of treatment (Goldstein, Heller & Sechrest, 1966). Finally, as Frank (1973) has pointed out, all schools of therapy indulge in role induction at the beginning, plus subtle and overt communications aimed at fostering patients' faith in a cure. One study showed that 20-50% of both participants' early remarks were directed toward clarifying the primary role system (Lennard & Bernstein, 1960). A standard instruction used to introduce desensitization (after Paul, 1966) exemplifies this practice in behavior therapy: "This procedure has been found very effective in the treatment of many types of fears, and we have used it successfully in the past with people who have fears like yours" (Morris, 1975, p. 238).

B. Social Psychology Derived Techniques: Manipulating Prognostic Expectancy

1. *Enhancing therapist attractiveness.*

Social psychology laboratory studies carried out by Goldstein and colleagues provide a rich data base for another approach to manipulating clients' expectancy of success, especially through arousal of therapist attraction. Based on the premise that psychotherapy represents an interpersonal influence situation, numerous techniques were designed for enhancing the quality of the relationship, "hooking" the client in the initial encounters, opening up channels of social influence and short-circuiting resistance. Recently, Goldstein (1975) summarized the complex functional relationship between the several "relationship-enhancer" methods (client structuring, imitation, conformity; therapist status, empathy and warmth; client-therapist matching; etc.) the therapeutic relationship components which they influence (liking, respect, trust), the consequences in terms of client influencibility, communication and openness, and the final outcome of behavior change.

Early in their research, Goldstein, et al. (1966) noted that resistance was a function of the communicator's characteristics, and that the personal attributes of the communicator affecting reception of the message were vividness of personality, status, and the expertise attributed to him. Trait structuring was a device for directly influencing perceptions of the communicator. The client was given explicit information describing positive characteristics of the therapist, how attractive he is as a person, how he will be liked by the client, or his similarity in personal background and life style. It was found that the greater the change-agent's perceived expertise, the greater his effectiveness in altering client behavior and beliefs. Apparently, a statement is more fully accepted and acted upon when the recipient believes it comes from an expert or high status person than when its source is a person of low or unknown expertise. Moreover, Greenberg's studies (1969, 1971) revealed that structuring a therapist as "warm and experienced" versus "cold and inexperienced" results in higher attraction, more receptivity to influence, positive evaluation of work and willingness to meet with therapist. In general, structuring the client with such information was found to diminish covert resistance, increase self-descriptive talk and favorable prognostic expectancies, while opening the client to the content of his therapist's communication (Goldstein & Simonson, 1971).[1]

2. *Matching and pairing to increase role expectancy congruity.*

A final approach capitalizes on assessed characteristics of client and therapist to arouse prognostic expectancies. Matching of patient with practitioner on some personality dimension, personal characteristic, attitude, shared background or preference, facilitates an initial perception of interpersonal complimentarity, role congruity and the appropriateness of help being offered. As suggested in section II, applicants enter with well-defined preferences and anticipations with respect to therapists' attributes and methods (c.f., Rosen, 1967; Ziemelis, 1974). Assessing preferences and basing therapist assignments on them can be an effective tactic to manipulate expectancies, increase compliance and continuance, and contribute to behavior change.

A number of investigators have sought to appraise the efficacy of matching and to determine the dimensions on which matching is worthwhile. Goldstein (1975) includes this approach as one of the "relationship-enhancer" methods. He suggests identifying the characteristics of client and therapist relevant to how well they relate, measuring them, then matching participants into optimal pairs based on these measurements. Optimal matching for Goldstein would occur along several dimensions: roles; confidence in positive results; similarity of sociocultural, racial and economic backgrounds; similarity in use of language, conceptual complexity, extroversion-introversion and social awareness; and degree of reciprocity in terms of need to offer and receive inclusion, control and affection. Carkhuff and Pierce (1967) concur with respect to race and SES; helpers who are most different from those they seek to assist in these respects have the greatest difficulties effecting constructive changes, whereas helpers most similar have greater facility for appropriate aid. Mendelsohn's body of work relied upon the Myers-Briggs Indicator (measuring cognitive-perceptual orientation based on Jungian life-style terms) to test personality similarity between client and therapist (Mendelsohn, 1966; Mendelsohn & Geller, 1963, 1967). The confusing results appear to show that more dissimilar pairs stay together shorter, that similarity is a necessary but not sufficient condition for clients remaining in treatment, and those more similar to their therapists miss more sessions. Along similar lines, the Minnesota Multiphasic Personality Inventory was used by Carson and Heine (1962) to show that either extreme similarity or extreme dissimilarity impeded the therapeutic process. Sapolsky (1965), in contrast, focused on compatible orientations in interpersonal behavior and found that it was related to greater improvement in hospital treatment. However, Garfield (1971) and Meltzoff and Kornreich (1970) in summarizing this and other studies failed to find convincing support for the notion that client-therapist similarity in cognitive style or value system leads to improvement. The trend did show a curvilinear relationship, with moderate levels of similarity having optimal effects, yet some studies found that dissimilarity in the pair was better.

Recently, Dougherty (1976) administered a full battery of psychological tests and found usefulness in carefully planned matching procedures in terms of outcome. More significantly, Howard, Orlinsky and Hill (1970) found that pairing therapy participants by life situation (e.g., married, single, divorced, male/female, young/mature, family person, bachelor, etc.) significantly and differentially influenced the satisfaction experienced by the client. However, sometimes similarity in life situation led to satisfaction and sometimes it didn't. Individuals reported a rewarding experience if the counselor embodied a successful life adaptation in a situation which was similar to their own; it was less rewarding for them if they found their therapist facing similar problems in their own personal situation.

In perspective, two types of procedures may be employed at the onset of treatment to foster client perceptions of imminent success. The first involves manipulations of the therapeutic relationship to enhance the working alliance and avoid resistance. Findings from social psychology are applied to increase the therapist's interpersonal attraction and thus his influencibility, credibility and persuasion. Among the several relationship-enhancer methods, structuring information about the therapist, his status, skill and personality, is reportedly effective in promoting faith and positive prognostic expectations. However, as Brehm (1976) notes, there are dangers in relying exclusively on therapist attraction to promote client change and it is best to maintain subtle measures insuring perceptions of personal volition to avoid reactance. The second procedure involves identifying relevant dimensions on which clients and counselors relate, then following a matching process to devise optimal participant pairs. The selective assignment of clients to professionals based on preferences, anticipations, cognitive styles, background similarity, and so forth, wisely capitalizes on existing individual differences to increase the probabilities of a successful therapeutic alliance and to arouse prognostic expectancies.

V. Some Critical Issues in the Expectancy Literature

The previous discussion sought to provide a careful and critical analysis of the concept of expectancy. Initially, specific expectancy ideas were traced to their roots within the broader belief systems of the individual's sociocultural group. It was recognized how such beliefs are embedded within a system of thought and custom at the very core of culture, where they touch upon the network of interpersonal relations that bind communities together. Secondly, notice was given to the potential diversity in content of expectancy beliefs across cultural and socio-economic boundaries, as each group maintains a unique conceptualization of what healing entails. Thirdly, widely contrasting therapeutic encounters were pictured based on either met or unmet expectations; the full implications of the congruity-incongruity dimension for treatment performance and outcome were traced. Based on this presentation, a model of the expectancy process was developed. The model views expectancy as structured into three functionally interrelated components: cognitive events (cause-effect attributions) give rise to generalized, non-specific emotional and motivational processes (prognostic expectancies) and guide or disinhibit certain overt behaviors (compliance with therapist; trying out new behaviors). Finally, mechanisms were elaborated for either washing out pre-treatment differences in role expectancy or taking advantage of them as leverage for manipulating prognostic expectancies. These mechanisms were found within the role induction interview approach, the various methods for enhancing therapist attraction, and the client-therapist matching strategies. However, the strength and applicability of these findings and tactics must be tempered by certain problems and limitations which come to light in the discussion of expectancy. In this final section, certain key problematic issues involving methodology, measurement and ethics are illuminated.

A. Methodology

Issues of methodolgy revolve around problems associated with determining an internally valid assessment of the expectancy concept, the range of potential alternative explanations for what is being measured, and the often inconsistent and elusive findings regarding the influence of expectancy. The assessment of expectancy is difficult given the conceptual clouding that permeates this literature to date. Most writers have failed to recognize that what is being measured is determined by the stage in the expectancy process and the component (cognitive, emotional, behavioral) under consideration. Lorion (1974) points out that the cognitive aspects (actual beliefs) require thorough documentation through open-ended interviews to provide clues as to their actual composition. Furthermore, efforts must be taken to identify expectancy beliefs as distinct from similar concepts of anticipation, confidence, conviction, and trust, hope and wish (Cartwright & Cartwright, 1958). The motivational (affective) component or prognostic expectancy is frequently viewed as dichotomous, present or absent, but may actually be represented by a continuum with *degrees* of expectancy of improvement. This carries implications for finding the extent to which it is generated by different conditions and the need for sequential measurement (Kazdin & Wilcoxon, 1976). Moreover, identifying relevant conditions is a highly complex task given that expectancy of gain is potentially influenced by the face validity of treatment procedures, experiences during treatment interpreted as evidence of improvement, and characteristics of the setting, therapist and client (Lick & Bootzin, 1975).

Problems with experimental control and measurement circularity abound in the measurement of expectancy of gain. The central question is how to measure it independently of the outcome it is intended to facilitate since the client's expectation of benefit is partially a function of the treatment-induced improvement. Additional problems include the confounding of expectancy measurement with the effects of praise and feedback of improvement from the therapist, or simply the client's prediction of how much he will improve on the basis of the information at hand (Wilkins, 1973). Timing of assessment is critical and may directly contribute to the type of expectations found (Kazdin & Wilcoxon, 1976). For example, measurement at the beginning of treatment, after the treatment rationale has been given, may tap only the credibility of the rationale or reflect changes in expectancy that are not associated with the specific technique. Also, initial subject assessment may sensitize them to later procedures making them more critical and suspicious. Once treatment has begun or upon client termination, the expectancy report becomes a function of the actual change experienced, with those who have changed reporting higher expectancies of success.

The nature of the assessment device itself remains an issue. Self-report measures have been the dominant approach employed from the beginning. Yet early researchers discussed the inadequacy of these measures. Goldstein and Heller (1960) found divergent results on self-disclosure as opposed to behavioral levels of measurement, complaining that their subjects' verbal behavior seemed to be aimed at avoiding psychotherapy, pleasing the therapist and saying the "right" things. Earlier, Goldstein (1960b) had questioned the sensitivity of self-report measures that were global and multi-dimensional based on expectancy of personality change, and switched to estimates focusing on "target symptoms" alone. The weakness inherent in opinion scales and similar self-report measures filled out by subjects once during the course of treatment are summarized by Kazdin and Wilcoxon (1976) and Lick and Bootzin (1975). Among their criticisms they point to reactivity as a major concern. Transparent to the subjects, they respond to the demand characteristics of such questions rather than the treatment itself, rating the incredible as credible because it seems expected (c.f., Rosenthal & Rosnow, 1969). Moreover, measurement error is increased and the variability in expectations over the course of interventions is lost with inadequate, non-sequential sampling.

To avoid these assessment pitfalls, alternative methods and practices are available. First, the clients' expectancies can be measured several times during the treatment at points associated with different therapeutic stages. Lick and Bootzin (1970) recommend the use of simulator measurements, independent of therapy manipulations, where the client simulates a performance on the outcome measure after exposure to the rationale alone. This provides a direct measure of the demand characteristics or credibility on measures of direct interest. Demand charactertistics have been minimized in desensitization studies by using highly fearful subjects, those individuals who are unlikely to perform the feared behavior under conditions of social pressure alone. Extending this control procedure to the assessment of other clinical techniques may prove valuable as well. Role playing expectancy of improvement based on preferred or non-preferred therapeutic conditions is another approach suggested by Nau, Caputo and Borkovec (1974). Kazdin and Wilcoxon (1976) add that expectancies for improvement can also be tapped by unobtrusive or bogus measures of therapy performance. For example, McReynolds, et al. (1973) were able to assess expectancy of success without their subjects' awareness by employing a false measure of treatment outcome. The extent to which subjects changed on this bogus measure (a task requiring crossing out numbers) provided an index of treatment credibility. Finally, in reference to desensitization research, Borkovec (1973) suggests that to avoid expectancy being defined in a circular fashion, expectancy manipulations (ie., therapist's instructions) should be viewed as discriminative stimuli or demand characteristics influencing post-test fear behavior. Therefore, subjects should be told that the post-test is an assessment of the successfulness of therapy in changing their fear behavior, thus inducing an expectancy or high demand for improved overt behavior.

The principal alternative explanation for what is being measured in these studies is that it represents some social desirability response. (Meltzoff & Kornreich, 1970). This interpretation becomes particularly plausible when self-report measures are relied upon as previous criticisms point out. Frank (1973) observed that when a patient says he expects psychotherapy will work and is optimistic, then his symptomatic relief when tested at that time may be explained by the desire to appear consistent. As Friedman (1963) warned in the early stages of this research, once a client has made a prediction of improvement, his subsequent self-reports are likely to be under the influence of both desiring to make a good impression and wishing to appear consistent with earlier statements.

The final methodological note involves the stability and generalizability of the finding that manipulating expectancy of success does in fact have an influence on therapy and outcome. Kazdin and Wilcoxon (1976) in their thorough review of expectancy in desensitization investigations conclude that there is a small but non-significant difference in outcome whether subjects were told that treatment would or would not be effective. Wilkins (1973) concurs, citing numerous key studies which were unable to demonstrate significant outcome differences among groups receiving different expectancy instruction showed less improvement than groups receiving low or no-expectancy instructions. Although Lick and Bootzin (1975) characterize expectancy manipulations as important change mediators, their review of analogue studies attempting to manipulate this variable in psuedo-therapy conditions failed to find significant effects for expectancy-including instructions. Moreover, Goldstein (1971) criticizes his own efforts to induce therapist attraction and thus expectancy by noting that the manipulations rarely have their intended effect on non-YAVIS (Young-Attractive-Verbal-Intelligent-Successful), non-university populations. There is little evidence for their durability when they do work, they are limited to initial interviews and, without continuous therapist intervention, the effects of these attraction-enhancer manipulations wash out over time.

B. Ethics

The final problematic concern to be taken up involves the issue of ethics as applied to the behavior change process. The question of ethics always arises in those situations where there is an inequality of power between individuals and one party seeks to exercise influence over the other. Ethical concerns are brought into sharp focus in the expectancy literature in those situations where the practitioner either capitalizes on the client's expectation of help to induce changes in attitudes, values and behavior, or through role induction and anticipatory socialization procedures, attempts to modify belief and value systems about therapy to bring them more in line with the therapist's preconceptions.

A significant issue is the recognition that cross-cultural counseling represents a process of intense culture learning. In teaching his model of therapy, the Western counselor imparts to his client culture-specific values and expectations about human behavior. He does so through a subtle process of shaping the client into a theoretically pre-determined conceptualization of his problem, and then suggesting the therapeutic steps which can be taken to "cure" such problems (Meichenbaum, 1976).

The notion that therapy represents an acculturation procedure for culturally diverse applicants is borne out by investigations showing that client progress in treatment is associated with their convergence towards the therapist's own set of attitudes, values and expectations (Appel, 1960; Bednar, 1970; Cook, 1966; Kessel & McBrearty, 1967; Meltzoff & Kornreich, 1970; Vaughn & Burgoon, 1976). Conversely, if the client refuses to accept the therapist's world view (resistance), the therapist loses leverage in the social influence encounter and fails to form an effective working alliance (Kessel & McBrearty, 1967; Torrey, 1972). However, the shift in patients' personality, value and meaning systems in the direction of those of their helpers seems logical, given that clients model their therapists and judge self-improvement by increases in similarity (Sechrest, 1962). It also seems logical for therapists to view themselves as models, and gage client improvement in terms of approximations to the therapists' own throught and action patterns (Rosenthal, 1955; Welkowitz, Cohen & Ortmeyer, 1967). Albert Ellis is a prime example of a therapist actively communicating his values and treatment rationale, and judging successful outcome in terms of client receptivity (Ellis, 1962). His approach is to induce the client to internalize a "rational" philosophy of life, thus abandoning his "self-defeating" value system. By implication, those who are most desirable as clients, and will be termed clinical "successes," are those willing to recognize their world view in congruence with Ellis' Western-derived perspective and integrate a peculiar Eastern American (New York City) set of values.

Traditional response of practitioners to clients with divergent belief systems, those who are not psychologically minded or "good risks" for treatment, has been either to develop assessment instruments to screen them out of the system or to invest energy in pre-therapy training (Garfield, 1971). Goldstein (1971) terms the latter course the "evolutionary" approach, where research programs seek to develop new procedures for indoctrinating non-YAVIS patients into traditional forms of psychotherapy (e.g., Heitler, 1976; Orne & Wender, 1968). In contrast, Goldstein (1971) and others advocate shifting to a "revolutionary" approach, to design new and different intervention procedures for divergent client populations. Emphasis shifts to tailoring flexible treatment packages to approximate the unique expectations and life-style characteristics of these individuals (Garfield, 1971; Lorion, 1974). Kinzie (1972) notes that it is far easier for the therapist to make changes in his style of interaction, to exercise flexibility and creativity in the design of treatment in order to make progress, than to mold the patient into pre-conceived "good client" behaviors. Another option is to offer a variety of treatment modalities, and apply the one best suited to the client's expectations, rather than what's available or easiest (Beakeland & Lundwall, 1975).

With regard to designing psychological services for culturally diverse and non-Western peoples, Higginbotham (1976; in press) has recently offered a model based on four fundamental culture-assessment questions: (1) the problem situation as understood by the community of which clients are members; (2) culturally expected goals of treatment and criteria of ideal adjustment; (3) culturally sanctioned means of influencing social behavior; and, (4) culturally sanctioned sources or agents of behavior change. Modifying the therapeutic package based on culture assessment of expectations rather than manipulating pre-existing belief and value systems regarding the helping process, is a creative solution to the dilemma of ethics as well.

VI. Conclusion

In previous sections, expectancy was lifted up as a concept critical to understanding how culturally acquired beliefs impinge upon the treatment setting, the evolution of a client-professional working relationship, and ultimately come to influence case disposition. However, in light of the issues just reviewed—the problems of concept definition, measurement, and validation, and the sometimes fleeting effects of expectancy as a treatment manipulation—it is apparent that definitive statements of the expectancy process should be tempered with qualifications or held in abeyance. What is required to advance understanding of expectancy is continued critical appraisal of the components involved in the process, along with empirical probes aimed at establishing construct validity for both role and prognostic expectancy.

Furthermore, researchers should be mindful of the cultural perspective, recognizing that role expectancies vary predictably across group boundaries. Item selection for the construction of test instruments must follow from preliminary, open-ended inquiries of culture-specific information, along the lines suggested by Higginbotham (1976) above. Interestingly, an additional benefit of constructing assessment devices in this fashion is their potential use as tools for evaluating existing psychiatric services from the perspective of the client's community. Using multiple criteria, norms for community expectancies can be measured and compared with available treatment practices in an agency or institution. A high degree of overlap would suggest treatment accommodation to cultural beliefs and customs, while absence of congruity may reflect either insensitivity to cultural patterns or a deliberate effort to bring about changes in this aspect of social life.

While the critical issues raised in the previous section do provide the challenge to work towards additional conceptual clarity and empirical evaluation of the expectancy process, they do not detract from the main contentions of this review. Namely, it appears that: (1) the client's culturally acquired belief system about the nature of psychotherapy plays a significant role in the course and outcome of treatment; (2) there is explanatory and predictive value in using the concept of role expectancy as a link between client culture and client behavior in therapy; (3) it is important to assess role expectancies first, and then either accommodate therapy to them or bring them in line with predetermined treatment practices; (4) ethically, it is preferable to tailor therapist actions to client expectancies; and, (5) unmet expectations frequently result in clinical failure, while hope, faith in success, and compliance with instructions follow from expectancies congruent with the actual treatment encountered. In essence, when cultural differences separate patient and healer, efforts to change human behavior fail in the absence of provisions for accommodating psychotherapy to role expectancies—a condition necessary for eliciting positive emotions and the motivation to try new, hopefully more adaptive behaviors.

FOOTNOTES

[1] Several of the specific suggestions laid down by Goldstein, Heller, and Sechrest (1966) for enhancing attraction have come under sharp attack by Sharon Brehm (1976). She noted that the recommendation to induce dissonance through overcompensation provided the patient for therapeutic participation is an application of equity theory (Adams, 1963) and directly opposite to what dissonance theory would predict (ie., the more effort and the fewer the rewards, the more attraction to the therapist should occur). Secondly, the recommendation for dissonance induction by patient participation in overt behaviors discrepant with resistive behavior takes the therapist far afield from the behaviors he actually wishes to focus on and is grossly inefficient. Moreover, the *a priori* assumption that attraction to the therapist and therapeutic situation is a cardinal prerequisite for successful therapy is called into question. Brehm offers evidence indicating that a client's lack of attraction toward the therapist is not necessarily a handicap and can be used to facilitate change.

The alternative line of thought based on reactance theory (Brehm, 1966) posits that because attraction produces social pressure to conform, it can also make that social pressure too strong and, in conjunction with an important freedom that is being threatened, create psychological reactance and the reduction of influence. As Brehm (1976) points out, although attraction may be necessary to elicit initial performance of a behavior, if it is kept high it may reduce the strength and stability of the desired behavior change. That is, attraction to the therapist can act as a justification for performance and reduce attitude change and the likelihood of engaging in the behavior without the therapist. Perception of personal volition will prevent reactance and facilitate dissonance arousal. To avoid reactance, Brehm recommends avoidance of overtly strong social pressure in therapy and commands to the client, emphasizing instead joint, consultive decision making as suggested earlier by Meichenbaum's work.

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SOME EPISTEMOLOGICAL AND METHODOLOGICAL ISSUES IN THE DESIGN OF CROSS-CULTURAL RESEARCH

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The burgeoning of cross-cultural research in the past couple of decades has created a situation in which the conceptual waters have been muddied. All too often cross-cultural projects are undertaken as matters of convenience with little or no attention paid to the epistemological and methodological issues involved. To be sure, some recent works (e.g., Brislin, Lonner, and Thorndike, 1973; Price-Williams, 1975) have examined a variety of topics germane to transcultural comparison. Still, the research conducted, as often as not, seems to reflect varied relative degrees of sensitivity to the qualitative differences in the nature of cross-cultural research as compared to standard social science research.

Part of the problem in engendering coherence in approaches to cross-cultural research lies in the fact that investigations emanate from a variety of disciplinary wellsprings. Moreover, transcultural studies are often looked upon askance within disciplinary bounds. It is probably accurate to say that cross-cultural research is regarded as a marginal activity in a number of disciplines. The difficulties in acceptance of this legitimate sphere of investigative endeavor are compounded by a number of factors. First, as noted above, transcultural studies have at times represented little more than the fortuitous confluence of a researcher, an idea, and a novel population. Thus, some have come to view the term "cross-cultural" as a euphemism for poorly conceived (or, indeed, unconceived) research problems. Secondly, the relegation of cross cultural investigations to the disciplinary hinterlands is reflected in the diverse array of sources for relevant literature. The serious cross-cultural researcher must keep abreast of developments in a number of areas and journals. This state of affairs is likely to further alienate the cross-culturalist from his or her colleagues in that they will quite often no longer even share a common literature. Finally, it should be noted that because cross-cultural research is not limited to one domain of behavior (e.g., abnormal psychology, economic anthropology, cognitive psychology, political anthropology), much literature appearing under the rubric of "cross-cultural" work will be of tangential relevance to individual investigators.

Little of the work published thus far has given explicit recognition to the requisite multidisciplinary nature of transcultural study (see the paper by Dinges in this volume). Two disciplines most intimately involved in the cross-cultural endeavor are anthropology and psychology. The aim of the present paper is to examine some epistemological and methodological issues pertinent to cross-cultural research from a bidisciplinary perspective. The authors are an anthropologist (SFW) and a psychologist (BJS). While we have used as major subdivisions the phrases "epistemological issues" and "methodological issues," we recognize that *problems* often shade from one area to another. Thus, aspects of our discussion might appear to be misplaced as regards section. We ask the reader to bear with us and remember that it is the problems (and not their taxonomy) that are of importance here.

An Examination of Some Epistemological Issues

A number of epistemological issues are engendered by the difficulties of doing comparisons between (or among) peoples with homogeneous backgrounds that differ from the homogeneous backgrounds of others. Some of these and cognate issues may be organized under a general heading regarding the general vs. the particularistic focus of the research enterprise.

Some of the concepts examined in this section are not uniquely epistemological in the cross-cultural context. Some familiar concepts are, however, subject to a new nuance of meaning in a transcultural setting. Thus, while some form of universal-particular dimension is relevant to most social science research, it is especially critical cross-culturally because of the variety of issues with which it interfaces there. Furthermore, there are other topics which go to the very heart of cross-cultural comparisons. Thus, for example, there is a common and important distinction made between two types of independent variable (the one for which the researcher wishes to determine an effect). Manipulated and natural-state independent variables differ in the internal validity which they afford an investigation (Cf., Campbell and Stanley, 1966). Transcultural research almost invariably uses at least one natural-state independent variable (i.e., culture or nation) and consequently encounters a virtually invariable difficulty with clean (i.e., internally valid) interpretation. The reality is, then, that the cross-cultural researcher must develop methods to deal with this potential pitfall. Although space is insufficient for a detailed treatment of causation and correlation (to which the distinction between the two types of variables relates), thoughtful discussions appear in Plutchik (1968), Raser, Campbell and Chadwick (1970), and Köbben (1970a).

Universal vs. Particular Orientation

As noted by Köbben (1970b), there has been an enduring controversy in many disciplines over the nature of research foci. Kant, he observed, devoted a chapter in *Critique of Pure Reason* to a discussion of two major orientations. Some investigators seek the homogeneity or regularity in divergent phenomena while others focus on the specific interrelationships within a particular phenomenon. A number of issues germane to cross-cultural research are groupable distinctions reflecting this basic dichotomy.

Rohner (1975), in the first chapter of his book, presents a number of characteristics which he feels are requisite for a universalist approach. First, he suggests that universalist research must be comparative in nature. Universal principles cannot be demonstrated on the basis of data gathered from one or two distinct groups (Cf., Berry, 1969). Second, Rohner argues that a universalist study must be nomothetic or generalizing in focus. The suggestion is that the *similarities* in divergent structures are the issues which demand attention. The final criterion for universalist inquiry is that it be scientifically acceptable. This demands intersubjectivity (Kaplan, 1964) in the results of investigations. Such intersubjectivity, while certainly a desideratum, may seem to be no sinecure for certain social scientists (e.g., the ethnographically oriented anthropologist). Koch's (1964) discussion of minimal language communities is pertinent here. According to his discussion, intersubjectivity would not imply that results are replicable by all who seek to replicate them. Indeed, in certain cases it is conceivable that a language community consisting of only two people might constitute the necessary intersubjectivity.

Emic-etic distinctions. One of the most often used distinctions in cross-cultural work is that made between emics and etics. It should be noted at the outset that various authors have radically different defining characteristics for these terms (e.g., Brislin, 1976; Harris, 1976). Moreover, the terminological equivalences typically mask genuine conceptual disparities. The authors are currently preparing a paper comparing the major usages of these varying conceptualizations and detailing the methodological ramifications which emanate from them. While space is insufficient to summarize that report here, one of the most typical definitions of the emic-etic dichotomy bears scrutiny here because of its relevance to the universalistic-particularistic approaches.

A number of authors (e.g., Berry, 1969; Triandis, 1972; Brislin, 1976) basically see emic inquiry as ferreting out monocultural principles of behavior and etic investigation as a search for pancultural principles. This view heavily influences much of current cross-cultural research (e.g., Davidson, Jaccard, Triandis, Morales, and Diaz-Guerrero, 1976), especially in psychology. Where a particular study falls on the emic-etic dimension tells us something about both the putative thrust of the research and the methods which have the greatest potential for yielding meaningful information on the topic at hand.

Comparativist vs. structuralist. A controversy which has long raged within anthropology and (to a lesser extent) history has involved those who seek their knowledge via examination of the configurational pattern *within* the object of study (e.g., culture) and those who seek regularities *among* a set of such objects of study. Köbben (1970b) has called the former type of researcher a structuralist, configurationalist, or, more simply, a non-comparativist. The latter type of investigator has been dubbed a comparativist or, according to Rohner (1975), a universalist. Transcultural research is by nature comparativist (see Brislin et al., 1973). The controversy is replete with recriminations among the various parties to the dispute. Basically the structuralists argue that comparison does violence to the desideratum of generalization via its ignoring of context. On the other side, comparativists contend that true generalization cannot be based on the intensive analysis of a single case. Köbben's (1970b) reasoned examination of the charges and countercharges led him to propound the argument that both approaches are necessary for a viable anthropology.

A related issue, seemingly recrudescent with every renewal of the discussion, is the notion of functional equivalence. Psychologists (e.g., Berry, 1969) and anthropologists alike (see, e.g., Rohner, 1975) have promulgated the necessity of functional equivalent behaviors and/or institutions as proper grist for the cross-cultural mill. Still, it is not always an easy matter to decide whether a behavior or an institution serves the same purpose in two cultures. Moreover, as Lowie (cited in Köbben, 1970b) has so incisively observed, even equivalence of purpose does not assure true equivalence. Thus, institutions and behaviors serving exactly the same purpose in two cultures may differ widely on the relative emphasis or weight accorded them by the people *within* each of these cultures. It is clear, then, that some kind of equivalence must be established for phenomena studied transculturally. What is unclear, however, is the most profitable and valid way or ways of assessing unequivocal equivalence.

Idiographic or nomothetic focus. Until his death in 1967, Gordon Allport was one of the chief proponents of the idiographic approach in psychology. Allport contended that the intense study of the individual was demanded in order to understand the existential richness of the unique human being. In large part, he argued, more atypical methods focused upon the individual [illegible] required. This position may be contrasted with that of the extreme nomotheticist, e.g., Hans J. Eysenck, the British psychologist. In the extreme, the nomothetic position would hold that all important aspects of human functioning are quantifiable and hence measurable. The unique individual, then, is defined as a point in n-dimensional space. Although the psychological study of the individual typically employs neither of these approaches to the exclusion of the other, both are probably necessary in cross-cultural work because concepts may have quite different meanings in various cultures. Both idiographic and nomothetic techniques appear to be necessary to unearth such differences. The paper by Sharma in this volume suggests a consistency with this notion in the substantive area of anxiety research.

Analog vs. digital descriptions of behavior. An issue related to both the comparativist-structuralist polemic in anthropology and the idiographic-nomothetic controversy in psychology has been cogently presented by Spence (1973). That author has argued that our attempts at quantification for measurement purposes may actually prove more disruptive of the phenomena of interest than can be recompensed through the increased rigor generally associated with quantification. Moreover, the increased rigor and precision often viewed as invariable concomitants of quantification may themselves be spurious. In any case, Spence's major concern is with the possibility of gaining an understanding of behaviors whose natural flow and order we abort in order to measure them.

Analysis: A common thread. A recurrent theme in the epistemological presentation of universalistic-particularistic approaches to cross-cultural study centers on the importance of context. Protagonists in the configurationalist camp in anthropology (e.g., ethnographers), the idiographically-oriented researcher and the practitioner concerned with analog measurement all take a basically Gestaltist view that context, pattern, configuration—the total picture—is the *sine qua non* of research which will result in "true" understanding. The cross-cultural psychologist and hologeistic anthropologist cannot, of course, afford to accept the extremist versions of these viewpoints. These researchers are staunchly comparativist and nomothetic and often, perforce, digitizers. Many (e.g., Berry, 1969; Price-Williams, 1975; Rohner, 1975) seem to be sympathetic to the middle-ground espoused, for example, by Köbben (1970b). All strategies are important to the discovery of valid principles of human and societal functioning. Although context or Gestalt may be important, safeguards are available to assure the preservation of meaning when comparison items are taken out of context. In addition, comparative nomothetic work is probably necessary to the establishment of disciplines smacking less of literature and more of science.

To the authors' knowledge, there has been little empirical work comparing the explanatory power of the universalist approach with that of the particularist approach. One research area which does, however, come to mind is that of clinical vs. statistical prediction (see, e.g., Meehl, 1954). A series of studies in this area compared the diagnostic skills of clinicians with quantitative decision rules which could be applied by anyone. The studies, then, might be viewed as a fairly direct head-to-head competition of nomothetic and idiographic orientations. The area is a complex one and caution must be exercised in interpreting the results; but there was, nonetheless, a surprisingly uniform set of findings. Clinicians generally were unable to improve on the classifications made according to decision rules.

Finally, it should be noted that the virulent criticism notwithstanding, the tolerance of a comparative research strategy informed by the epistemological issues adds another weapon to the armamentarium of developing disciplines.

Epistemological Issues in the Research Process

The conclusions reached by cross-cultural analysis are heavily dependent upon both the confidence that can be placed in the gathering of data and the inferences made from such original data. Therefore, the epistemological issues present at each step of the research process may have considerable influence on the validity of generalizations established by cross-cultural research. The aim of "objectivity" in social science may well be a red herring. More realistic goals might include the establishment of a reliable data base and the articulation of biases which could affect the validity of data reports. We will discuss some of the epistemological issues which relate to these goals in this section.

The cross-cultural research process may be thought of as the sifting of data through a series of classificatory filters to the point where generalizations remain. For example, taxonomic criteria may be implicit in the language, explicitly defined as part of the theoretical orientation of the research, implicity codified in analytical concepts, or part of the value system of the researcher. Unless the cross-culturalist is aware of the types of classifications which affect his conclusions, seemingly innocuous aspects of categorization may accumulate and harm the validity of his data.

Data collection and codification. The first stage of the research process usually begins with an investigator who describes the behavior of people in a particular situation or setting. These reports become the basis for subsequent analysis not only by the original investigator but also by other interested researchers. Because of the hierarchical nature of the filters in the research process, errors made at this most basic level are carried over into later phases and thus become seriously compounded. Therefore, it is essential that the multifold dimensions of classification present at this phase be elucidated as potential sources of bias.

The status of the investigator includes his position in his own society as well as his place in the research setting. It is important for the researcher to recognize that participants in the research setting may not interpret his status in the same way that he does. For example, an anthropologist may enter a community as a scientific observer but be regarded as something else, e.g., a witch or an inquisitive and unwelcome government official. Age, sex, marital status and occupation (missionary, government official, scientist, etc.) are among the status criteria which may be relevant to members of the research setting and thus constrain the researcher's access to information. If he is not clear about the status which the members of the community assign to him, the investigator may be unable to evaluate the credibility of the information which he does receive. The assignment of a status role to the researcher by the members of the culture illustrates that it is difficult for a researcher to maintain a totally passive stance in a new cultural context.

The disciplinary affiliation of the researcher is particularly germane to the research setting for it channels his role in the community. A dominant orientation which anthropologists express in the field is that of the participant observer—the scientist who observes behavior but hopes he is transparent enough to not influence the chain of events in the surrounding milieu. (For a more detailed account of the problems of participant observation, see the paper by Eckermann in this volume). He assumes that there is a natural order of events which he can discover if he remains among the group for a long enough time. His research revolves around his living with the natives and observing their behavior in a variety of situations. Thus, he seeks to describe the natives in a variety of situations with minimal intrusion on his part. Ethnoscientists represent the extreme manifestation of this view for they want to observe the situations in terms which are acceptable to the natives themselves. On the other hand, psychologists generally enter the field with a very specific hypothesis to test. Their stay may be limited and the extent of their contact with the natives confined to their research problem. Furthermore, they may structure the situation by setting up experimental procedures and taking an active role in encouraging the natives to participate in the experiment. In sum, the disciplinary orientation of the researcher may not only channel his interest in the type and range of situations which he selects for observation but also his role within them. An anthropologist may adopt a more passive role and attempt to describe a wide variety of naturally occurring situations. The psychologist may adopt a more active role in the community and structure a limited number of situations from which he hopes to derive information.

The researcher's choice of data collection techniques can also exert an important influence on the information he receives. Language plays a crucial role in many forms of information elicitation procedures—in questionnaires, interviews, and observations. If a researcher relies upon an interpreter, he also relies upon the translator's evaluation of the equivalence of terms and appropriate usages for the verbal expression of his thoughts. Thus, a translator adds a pervasive filter to a large portion of the research process. Since language codifies a good deal of cultural information, speaking the language becomes one dimension of identification with the community and a sound basis for discovering important nuances of the natives' conceptual organization of their behavior. Not only must the researcher understand the applicability of words to signify particular objects; he must also be able to pinpoint the array of meanings which are attached to the behavior which he observes. His misuse of terminology could inadvertently skew his findings.

Often investigators rely upon informants or indigenous personnel to aid them in their research. Since an informant is a member of the culture, he occupies a particular status and role within that context which may influence the selection of information which he offers to the investigator. In addition, he may adopt the stance of an observer rather than a participant in his own culture. It is essential that the researcher discover whether his informant is providing him with information in terms of his own categories or is communicating with the researcher via the researcher's categories. Without an explicit recognition of the source of the data, it would be difficult to pinpoint the nature of the biases involved.

Access to information may be further limited by the investigator's length of stay. Naroll (1962) has shown that length of stay may enhance the probability that a researcher will be given valid information about subjects such as witchcraft and sexual conduct. Thus, a longer period of residence with members of the research setting would be a more optimal condition for eliciting a thorough knowledge of the community context.

Conceptual categories are also an integral part of the disciplinary orientation and value system of the investigator and thus influence the way that primary information is codified in the research report. For example, anthropological concepts may be more descriptive of cultural context and take individual variations less into account than psychological concepts do. Disparate types of behavior may be organized into such a conceptual scheme if descriptions have an abstract focus. The values of the investigator may also channel the way in which the information is processed. They may be expressed in the language and lend an evaluative component to the description which is unwarranted.

Since a researcher cannot describe all that goes on about him, some form of selection of information will be operative in the recording of the data. The goal of his research will influence the range of situations he includes in his descriptions. If behavior which he observed is not relevant to his research aims, he may omit it from his report. Because of his own cultural background, he may find contrasting aspects of another cultural context fascinating and describe them in more detail than those with which he is familiar. Consequently, his description may lead the reader to infer that these aspects are more important in the culture than they really are. The omission of details uninteresting or irrelevant to the researcher makes it difficult for the hologeistic[1] researcher to know whether the specific data is present but not described or actually absent in the culture.

Data retrieval. The discussion thus far has focused on epistemological issues relevant to the field situation. The importance of data codification at that stage is magnified when subsequent researchers attempt to retrieve information from these primary research reports. The degree of its importance depends upon the reliance which the cross-culturalist must place on information which he has not himself collected. The cross-cultural psychologist who conducts experiments in sometimes as few as two cultures or the anthropologist whose interest lies in controlled comparisons (Eggan, 1954) between communities need not rely upon data which he has not collected. However, if they do need to incorporate information from other sources in their analyses, the limited scope of their comparisons facilitates a more thorough knowledge of the cultural context. On the other hand, hologeistic researchers, because of the large size of their samples, are more likely to utilize data which they have not themselves collected. This does not mean that such researchers are uninterested in the mechanics of the field situation. In fact, comparativists may be more concerned with the accuracy of data reports than the original researchers because such descriptions constitute their major data base. Thus, the scope of the cross-cultural project will influence the way in which the researcher tries to retrieve information for his analysis. The cross-cultural psychologist may more easily deal with specific aspects of the cultural contexts within which the investigation took place. For example, the equivalence of concepts or translation difficulties may be examined in detail. While the hologeistic researcher may be interested in such problems, he may be unable to effectively deal with them because of incomplete or inaccurate primary data reports. He thus resorts to further classification of the data in order to systematically retrieve it.

The process of data retrieval in hologeistic studies generally consists of the selection of a sample of societies for which information is desired, the construction of a code for classifying the information, the selection of source materials pertinent to the societies in the sample, and the coding of information from these sources into symbolic form appropriate for computer storage. Therefore, additional sources of bias may be introduced in the very process of information retrieval, because each of the aforementioned aspects of hologeistic studies relies upon the judgment of the comparativist and the coders in systematizing the data. For example, the comparativist must define the criteria by which he selects a group for inclusion in his sample of societies. Sampling raises fundamental theoretical issues in anthropology, e.g., the definition of culture and the designation of an appropriate unit of investigation for cross-cultural purposes. Code construction involves decisions concerning the applicability of categories to a variety of cultural contexts, whether categories are "natural" or artificial, and the numbers of categories sufficient to adequately represent the data contained within them. The selection of sources for coding may be influenced by the languages in which the reports were written and the investigator's familiarity with them. Further classification occurs in the coding process when the coder must read the sources and select information from them which he judges to be pertinent to the categories constructed by the investigator.

Data analysis. When the cross-cultural researcher finally has his data in a form which he can analyze, he is faced with the formidable task of explaining his results. It is at this stage that he must deal with the accumulated fund of classificatory schemes introduced at previous stages of the research. He must decide how the conditions of data collection and retrieval have influenced his findings. He must also take into account his own disciplinary bias, ethnocentrism, and value orientation in explaining his findings.

An Examination of Some Methodological Issues

Some methodological suggestions for the research process are presented below. A subsequent section details some commonly employed cross-cultural research designs and presents a new design approach suggested by the authors.

Methodological Suggestions for the Research Process

Cross-culturalists ambitiously try to generalize, explain and construct theories about human behavior. They do this in a variety of ways. One is to identify regularities and variations in behavior in different social contexts. A second is to test hypotheses about the relationships between these elements. A third is to explain the conditions under which particular configurations of elements are likely to occur. Thus, cross-culturalists focus less on the context from which the elements under investigation derive than on the systematic relationships that appear to be independent of context. This isolation of behavior from its context is particularly anathema to proponents of the idiographic approach. They argue that the uniqueness, pattern, and meaning of the element in question are lost. In essence, they see the heart of their traditional disciplines sacrificed for a few dry bones of data that take the form of generalizations that are trivial, tautological, or boring. However, it would be difficult to maintain that individuals and human societies are so unique that they have nothing in common (Cf., Köbben, 1970b). Cross-culturalists only want to sort out patterns of behavior which societies in their sample share and those which they do not. Thus, a cross-cultural study complements rather than opposes a single case study (Cf., Rohner, 1975). The data base on which cross-culturalists rely is drawn either from case studies or experiments conducted in a particular cultural context. The sophistication of the hypotheses which the comparativist can test depends upon the accuracy and completeness of primary data collection procedures followed in the original cultural setting.

Data quality control. As we noted earlier, the articulation of bias in reporting and the establishment of measures of reliability for the reports are realistic goals for data utilized in cross-cultural research. Naroll (1962) is particularly concerned with the whole issue of the reliability of reporting in cross-cultural research—the general trustworthiness of reports on which conclusions are based. He suggests a new method for dealing with these biases—an approach which he calls data quality control. The technique depends upon the assumption that some records are made under conditions of higher apparent trustworthiness than others. The essence of the method is the comparison of reports made under better conditions with those made under presumably worse conditions to discover whether the data seriously differs between the two. If it does, it is incumbent upon the comparativist to methodologically control the bias and take the source of error into account in his explanation of the relationships that he uncovers in his data. Naroll specifies six main control factors which are relevant in data quality control tests: "(1) collection of specific case reports by the ethnographer, (2) use by the ethnographer of direct observation and personal participation in an ongoing culture as a major source of field data, (3) length of stay in the field among the people studied by the ethnographer, (4) familiarity of the ethnographer with the language of the people studied, (5) role of the ethnographer among the people studied . . ., and (6) explicitness and generality of the ethnographer's report on the trait in question, with the concomitant presence or absence of a need for inference by the comparativist in order to classify the report." (1962, pp. 15, 16) Thus, his method helps the researcher to circumvent some of the potential problems of bias mentioned above in the discussion of epistemological issues. It provides guidelines for selection of sources which are more likely to contain valid information than others. The descriptions of participant observers are given precedence over those of people who have had short and perhaps superficial acquaintance with the culture. Implicit in this recommendation is a recognition that quality in cross-cultural research is predicated upon the quality of original data collection procedures. Simply, those sources which reflect excellence are preferred over those which do not. Ranking of sources is a valuable aid to the coder of cultural materials. If there is a conflict in information found in two different sources, there are definite guidelines along which one can judge which report is more likely to contain valid information. Although the utilization of control factors may introduce a selection bias into the data retrieval process, it is explicit and defensible.

In addition to the controls which Naroll suggests in the selection of sources, it would be advantageous for the researcher to read at least two reports, where possible, on the same group in order to assess the consistency of the information. This would provide some form of reliability check on the information he finally decides to use.

Methods for multilingual comparisons. When the cross-cultural researcher undertakes the study of groups with a language different from his own, he is immediately faced with problems of translation. All phases of the research sequence demanding a response from the culture-bearer must be translated into the target language. A number of thorough discussions of the problems with and methods of translation already exist (see, e.g., Werner and Campbell, 1970; Brislin et al. 1973). Here we will outline major translation techniques and discuss their adequacy.

Back-translation is a much-favored technique designed to ensure the adequacy of the passages recast into another language. A bilingual translator converts the original communication into the target language. Another bilingual then independently translates back into the original. Even individuals without skills in the target language can make judgments regarding the goodness of the translation by comparing the two original language versions.

A second technique employs bilinguals as respondents to both the original and the target language version of a communication or questionnaire. Response discrepancies between the two language forms by the bilinguals are suggestive of passages with low quality translation. Such passages are then scrutinized and reworked.

An often unsatisfactory approach has multiple translators working independently and then deciding, often in concert, on the "best" translation. This "committee" approach is prone to a number of biases (Cf., Brislin et al., 1973). Finally, these techniques may also be employed in combination with one another.

At the outset of any discussion on the adequacy of translation, the bicriterional nature of the judgment should be specified. First, an adequate translation must be readable. Second, it must convey the intended meaning. Any translation may fail either or both of these tests.

Two recent studies (Lowry and Marr, 1974, 1975) have employed Darnell's (1970) clozentropy technique as a measure of international communication comprehension. The technique capitalizes on the redundancy of language and can be profitably employed to assess the readability of a communication. To the authors' knowledge no one has yet suggested this technique as an objective means of evaluating the readability of a translated passage. A translated passage could be tested on a group of people in the target culture. Variability of response in the clozentropy procedure would be indicative of low readability.

Language differences often make direct meaningful translations impossible. A cultural decentering procedure in which both language versions are fair game for modification has been suggested as a means of attenuating this problem (Werner and Campbell, 1970). Still, caution must be exercised in employing decentering procedures. The decentering process attains common meaning by *leveling out* the emic meanings of both language groups. Thus, nuances of meaning are lost for both groups. Decentering is a subtractive procedure. When a subtractive procedure is employed in certain contexts, its effect is problematic. For example, if the meaning of a construct (say, anxiety) derives from linguistic nuances, the decentering procedure employed in the translation of a measure of that construct could wreak havoc on its content validity for both language groups.

The role of the researcher. The traditional mode of primary data collection in anthropology has been the case study approach in which a single investigator explores aspects of a particular cultural context. Further, he may rely on information provided by one or a few informants. Thus his disciplinary bias, value orientation, and the nature of his relationship with his informant(s) may bias the data obtained. One potential way of countering these sources of bias is to collaborate with one or more investigators from a discipline different from one's own (see article by Elequin, this volume). This would add to the variety of data collection techniques and provide a check on the accuracy of observations made by the researchers. It could bring disciplinary and personal biases to the surface so that they could be explicitly recognized and dealt with in the research setting. Cross-cultural psychologists and anthropologists could profitably collaborate, combining elements of research design with unique anthropological elicitation procedures. If data were collected by more than one technique, the results of varied procedures could be compared for their consistency. If the conclusions were similar, then more confidence could be placed in the validity of the primary data.

While Naroll's scheme of data quality control stresses the participation of the investigator in the research context, there is the danger that the researcher may become too closely identified with the culture under study. It might become difficult for him to evaluate his research role and to adequately assess the behavior of natives with whom he interacts.

Code construction and coding. Cross-cultural codes are like questionnaires given to cultures. They are designed to facilitate the retrieval of information about patterns of behavior present in a variety of cultures. Bias can originate with the introduction of concepts which are ethnocentric or value laden. One way to counter these problems is to use a *series* of categories rather than a coder's judgment of the presence or absence of a particular form of behavior. Although the parsimony of a presence/absence scheme is attractive, there is also the danger of lost information when the behavior in question exhibits variation. The use of multiple categories allows the investigator to see more clearly the dimensions of the items being coded. Pretesting of preliminary categories can reveal possible ethnocentric biases implicit in them. Numerous exceptions to the categories could provide a basis for their revision so that they could accommodate a wider range of information. Pretesting helps reveal the incompatibility between the concepts of the investigator and those reflected in the data.

Coders should have some sort of familiarity with the coding categories via a pretest experience. Pretesting also provides an opportunity for the investigator to elucidate the coding criteria. The coding process may reveal concepts so vague as to be useless. Moreover, the more precise the bases for categorization, the greater the probability that there will be inter-coder reliability. Thus the expenditure of energy in code construction has payoffs for both current and future cross-cultural research.

Designs and Programmatic Research Approaches

In this section we present a number of designs and programmatic research approaches from a variety of sources. Typically these approaches have evolved out of the need of some cross-cultural researcher to come to grips with a number of the epistemological issues outlined above.

A programmatic approach for finding and measuring universals. Some years ago, John Berry (1969) proposed a programmatic research approach designed to disinter behavioral universals. Berry's starting point is functionally equivalent behaviors between the culture under study and one or more cultures known to the investigator. The researcher starts with either an *assumed etic* (a global classification which he feels may work in the culture under study) or an emic (local classification) from some culture already known to him. In either case, he starts with what Berry calls an *imposed etic.* This imposed etic should be constantly checked and revised to better align it with the natives' own categorical structure. If some of the original (imposed etic) structure remains, it has become a *derived etic.* The derived etic may then serve as an imposed etic in a new cultural setting. A derived etic which has survived the test of all relevant cultures (i.e., those with functional equivalence regarding the behavior under study) is a behavioral *universal.* These universals (or, indeed, derived etics) may then be measured in the relevant cultures once the conceptual equivalence (equivalence of meaning for the natives of the various culture groups) of the measures has been established.

The weakness of Berry's strategy lies in its dependence on functionally equivalent behaviors. Problems with that concept were outlined above. Berry adopts a scheme for assessing functional equivalence that was suggested by Goldschmidt (1966). This scheme, while specifying some reasonably rigorous criteria for establishing functional equivalence, does not deal with the problem of differential weight or emphasis of the behavior in two distinct cultures.

Triangulation. Rohner (1975) has suggested an investigative approach which he terms triangulation. The research strategy involves gathering data on phenomena of interest by employing both a number of methods and various measures within each method. Readers with a psychology background will recognize triangulation as a combination of multiple operationism and the multi-method approach designed to assess method variance which was presented in the seminal paper by Campbell and Fiske (1959). The method is costly and perhaps over-rigorous (Cf., Starr, 1973) for a number of purposes.

Structuring emic and etic measures. Brislin (1976; Brislin et al., 1973) has examined the work of Przeworski and Teune (1970). It will not be detailed again here. Suffice it to say that the authors advocated the creation of measures tapping both culture-specific items and items relevant to all cultures under investigation in a particular study. These latter, core (or idential indicator) items are regarded as etic in nature. Examination of the correlation matrices yielded by the different types of measures within and across various samples is both instructive and predictable. The problem with the technique lies in its potential for violence to the content validity of the measure under study. Because core items define the etic content and core items will only exist for statements relevant to all cultures being examined in any particular investigation, the etic is, perforce, subtractive. Thus, etics so defined will often be deficient with regard to content validity[2].

Graduating steps design. This design (or rather series of designs) is due to Price-Williams (1975). That author sees three major components of the experimental situation—the task, material, and the context. Each of these may be varied so as to be familiar or unfamiliar to research subjects. By systematically varying the components, a researcher can evaluate the effects of familiarity with each (or any combination) of them on performance (e.g., performance on a cognitive task).

A related design also useful in measuring the effects of familiarity on performance is the cross-over paradigm. Here, for example, two culture groups might be given both familiar and unfamiliar tasks. Resulting performances may be directly compared in terms of both familiarity and culture. The design is especially valuable in situations where "culture fair" tests are needed.

Factor analytic designs. Davidson et al. (1976) present an approach which typifies the work of Triandis. Here, measures are developed emically within a particular culture and results are factor analyzed cross-culturally. Etic items are those which emerge on a common factor across cultures. Problems with factor analysis aside, this type of design has two shortcomings. First, as was the case with the approach structuring emic and etic measures, the process is essentially subtractive. Thus, again, the etic measure will have questionable content validity. Second, the within-culture content validity of the emically developed measures may, in some cases, be poor and will almost assuredly be uneven among the measures developed in different cultures.

"*Layered design*." The "layered design" is actually a research *program* conceived by the present authors. Three stages are involved in the program. Most basic to the success of the enterprise is a solid ethnographic study which would provide data for hypothesis generation. The ethnography, then, represents a first step toward understanding at an emic level. The ethnography would be followed by nomothetic research developed and carried out at the emic level. These investigations might embody, for example, tests of hypotheses developed via the ethnographic studies. These first two steps might be carried out simultaneously in more than one culture. The final layer in this design would involve cross-cultural nomothetic research carried out in search of etics. As was the case with the studies comprising the second layer, the etic investigations would emanate from findings at the previous (i.e., second) level. Thus, the layering occurs because the research at any one level is a natural outgrowth of results of investigations at the previous level.

At first blush, the "layered" approach might appear to be both time consuming and costly. Further, some might argue that the return may not be worth the effort. Three points are apposite. First, it is the authors' belief that it is only via some energy expenditure that we can begin to understand other cultures to a point where we can meaningfully discuss and attempt to solve the knotty problem of equivalences of any sort (i.e., functional or otherwise). Second, many excellent ethnographies already exist and thus certain first "layers" have already been done. These ethnographies are not only available (e.g., in the Human Relations Area Files) but they are also often rated for quality by other professionals. Finally, it is only by undertaking programmatic research building on previous findings, that we can move efficiently toward an understanding of the world's multicultural complexity via methods of strong inference (see Platt, 1964).

FOOTNOTES

[1] In very general terms, the hologeistic method involves relating two or more variables as reported in ethnographies from all over the world. The number of ethnographies consulted is usually 100 or more. The social scientist using the method consults one of these original sources and records the ethnographer's comments about different variables, for instance, methods of child rearing and type of economy. There is then a coding procedure to put the observations into mathematical terms so that statistical analyses will be possible. The same variables are recorded for all the other ethnographies, and then they are interrelated to determine the degree of association among them. Some social scientists use the Human Relations Area Files (HRAF) which is a facility available at many large university libraries. The developers of HRAF have organized ethnographies into an easily-usable filing system either on paper or on microfilm, and they have noted where information about different variables can be found. Other social scientists prefer to search through the original ethnographies themselves to find the information they are looking for [Ed. - RB].

[2] The authors are correct in pointing out that etics would not be content valid for any one culture since etics would consist of aspects of a concept which exist in all cultures under study. However, proper use of the approach would include gathering emic aspects of a concept, that is, those aspects that are part of a concept within any one culture. Hence a combination of the etic and the emic would yield a content valid concept [Ed. - RB].

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INTERDISCIPLINARY COLLABORATION IN CROSS-CULTURAL SOCIAL SCIENCE RESEARCH

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Introduction

Considering the ambitious if not grandiose goals of much cross-cultural research, it becomes apparent that no single social science discipline is capable of handling the problems of theory, method, data, and interpretation within psychology. For example, Triandis, Malpass and Davidson (1973) identify at least the following reasons to conduct cross-cultural research: to check the generality of psychological laws; to increase the range of observations on variables of interest; to determine the variations found in subjective culture variables in different settings; to take advantage of natural experiments involving combinations of variables that cannot be obtained in the laboratory; to study the manifestations of psychological variables in different cultural contexts; and to study cultures for their own sake. Clearly, no one social science discipline can hope to undertake research with culture as a major independent variable without readily confronting the limitations of the discipline at some stage of research.

The inevitable, if not the imperative need for interdisciplinary collaboration in cross-cultural social science research has, of course, been anticipated by calls for interdisciplinary research on various social problems within cultures. Sherif and Sherif (1969) described what they viewed to be the irreversible trend toward interdisciplinary collaboration and coordination which was a necessity for the development of each of the social sciences, and not a matter of individual preference. They propose that the substantive issue of the validity of generalizations and theories required that each of the disciplines needed the finding of the others as a check on the validity of its own generalizations. For a particular discipline, the core problem of the interdisciplinary relationship is to determine what findings and what concepts it has to borrow and in what matters it has to be in transaction with other disciplines in order to insure the validity of its own formulations. It should be added that advances in cross-cultural research would strongly indicate that the validity of the generalizations and theories of all the social sciences are being challenged by findings which show the provincialism of past research (Brislin, Lonner, & Thorndike, 1973).

Given that interdisciplinary collaboration is a highly desirable and possibly even necessary strategy for cross-cultural research in the social sciences, one immediately is led to ask what we know of this enterprise. The answer, unfortunately, is not very much. There have been some successful efforts in the past and interdisciplinary efforts currently appear to be increasing in frequency, if not in quality (Guthrie, 1976). Unfortunately, it is difficult to locate literature related specifically to this topic and one looks in vain for a set of guiding principles or even hints which provide a trans-situational context within which to plan and implement projects.

This paper will attempt a preliminary examination of the most salient factors involved in implementing interdisciplinary cross-cultural social science research. Obstacles will be identified on the individual, group, and institutional /organizational levels and solutions will be proposed based primarily on my own and others' field experiences with project management strategies of observed or reported effectiveness. Needed research will be described which deals with providing answers to increased competence in the management of such projects.

Obstacles

There are several levels of obstacles to effective interdisciplinary collaboration which fall generally into individual, group and institutional /organizational categories. While they may be analyzed separately for their various contributions to the interdisciplinary enterprise, it is apparent that they operate in a much more dynamic way in actual programs of research.

The Actor Factor

One thing that almost all researchers with interdisciplinary experience seem to agree on is that the individual personalities of the participants *do* make a difference in the conduct of reserach. Theoretical problems of trans-situation stability of personality traits aside for the moment, certain individuals do better than others in interdisciplinary collaboration. As might be expected, it is not too difficult to identify the individual characteristics that are considered important in the collaborative process. They are much like what one might expect to be helpful in other human group endeavors. Reliability, flexibility, patience, etc. have all been identified by one or another experienced interdisciplinary collaborator as essential to successful outcomes. The total complex of personality characteristics needed for interdisciplinary collaboration in cross-cultural research has probably best been summarized by Jacob and Jacob (1977) as follows:

> Can one recruit a "critical mass" of collaborators with the motivation, resilience, thickness of skin, sensitivity to other's feelings, humility, patience, readiness to learn, and basic know-how in conflict avoidance and conflict resolution to hold the show together through four or five years of mounting tension and frustration?

Despite the seeming impossibility of finding such beknighted individuals, experienced interdisciplinary collaborators report comparatively moderate amounts of friction due to personality factors among the collaborators. Some have speculated that a process of natural selection operates such that cross-cultural interdisciplinary collaboration attracts people with goodly amounts of the human characteristics described by Jacob and Jacob. Others have observed that both the direct and fringe benefits of the process create an incentive for muting otherwise objectionable personal characteristics, and that a small group culture operates to prevent the misuse of status differentials for personal aggrandizement.

Something akin to natural selection also seems to be operating at the individual level of interdisciplinary collaboration. For example, those individuals who do not respond to the incentives of interdisciplinary collaboration or who have not been weeded out by a process of natural selection in the first instance simply fall away from the collaborative enterprise much like one falls away from the practice of religious beliefs that have ceased to provide important spiritual satisfactions.

Among the actor factors that create substantial obstacles to effective interdisciplinary collaboration in cross-cultural research are the lack of experienced and effective leaders of such programs. There are those who have demonstrated competence to be sure, but they are not common and they learned what they know about managing projects through years of experience and failure and frustration. If one wanted to design a sure-fire method of assaulting both professional and personal self-esteem, it might be wise to look for prototypes in attempts at interdisciplinary collaboration in cross-cultural research.

The author's most recent experiences in interdisciplinary cross-cultural research groups illustrate this point quite well. In each instance the objective was to develop a research project of mutual interest and significance for the group members. In groups of essentially similar composition in terms of research experience and expertise the outcomes were quite different, both in group productivity and the apparent satisfaction of the group members. In one instance, the authoritarian style of the leader quickly became a source of covert resistance for the group members, which led to fragmentation of interests and eventful disbanding of the group. In another instance, the intimidating expertise of a consultant was responded to by various group members by working out dyadic strategies outside of the group to pursue research interests. This was a particularly well-qualified group with many key figures from different countries interested in the topic area, yet the leadership style involved was not adequate to produce a joint effort that could capitalize on this potential. In both of these instances the informal interactions with group members revealed a deep concern that more was not produced, but no one was able to see a clear solution to the group dilemma.

Part of the reason for the lack of experienced interdisciplinary cross-cultural research leaders may be traced to the relative newness of the area. After all, interdisciplinary research in traditional academic institutions has been only recently strongly advocated and modestly supported. Adding a cultural dimension to interdisciplinary research must surely produce the image of a bewildering complexity of human and scientific considerations. And those who would embark on collaborative projects in the area must certainly give serious consideration to their personal careers (see section on "disciplinary constraints" below.)

Roose (1969) has observed that by and large interdisciplinarians in the social sciences are among the most creative and advanced thinkers who pose the cutting-edge questions which require interfacing of knowledge and approaches for their solution. One wonders if the same could be said of cross-cultural interdisciplinarians. Is the excitement or stimulation that heightens enthusiasm and enjoyment for scholarly efforts increased by adding the cultural dimension, or does it tip the balance in the direction of a stultifying information overload which sends would-be interdisciplinary cross-cultural researchers back to simpler scientific pursuits?

An Assortment of Myths

Among the more formidable obstacles to interdisciplinary collaboration in cross-cultural research are some myths which, although harmless on the surface, may lead to dashed hopes and increasing threats to self-esteem if seriously indulged. One of the more potentially destructive of these myths is what Campbell (1969), in another context, has called the Leonardesque aspiration; the goal of creating Leonardos (after Leonardo da Vinci) who are competent in all of social science. Training programs which attempt to produce multidisciplinary scholars who have mastered two or more disciplines are guilty of perpetuating this myth. One need only be part of an interdisciplinary cross-cultural collaborative venture on a relatively small scale to realize how quickly ignorance of other disciplines is exposed.

A closely related myth is that of unidisciplinary competence, or the image of scholars competent in one discipline. One need only attend a regional or national conference in one's own discipline to realize the limitations of their specialty in a discipline. While disciplines may have within them a collective competence based on a composite of within-discipline specialties that overlap, it must be recognized that this competence is not embodied within any one scholar (Campbell, 1969). However, the disciplinarian members of interdisciplinary collaborations are often viewed as comprehensively competent by their collaborators and may even delude themselves into believing that they are indeed omniscient, at least in their own domains. A similar myth-perpetuating process occurs when the interdisciplinary collaborators also happen to be from different cultures. Then they are often placed in the position of being both a disciplinary omniscient and a cultural omniscient. This is a deadly combination and difficult both to resist personally and to avoid imposing on others.

A particularly seductive myth has to do with the "true-believerism" in interdisciplinary cross-cultural research to provide answers to the pressing problems of the social sciences. Much like Kurt Vonnegut's (1974) *foma* (i.e., harmless, comforting untruths) one is led to believe that putting enough disciplines together to work on social science research questions that are approached cross-culturally will somehow produce superior outcomes. Considering current levels of ignorance in the various social science disciplines and the relative youth of the cross-cultural approach, it will be obvious that we are in danger of producing a *granfaloon* (i.e., a proud and meaningless association of human beings) in search of a *wampeter* (i.e., an object around which the lives of otherwise unrelated people revolve) to borrow still other concepts from Vonnegut.

Disciplinary Constraints

On the utterly sober side, it must be recognized that there are a variety of constraints on the members of the various social sciences which mitigate against effective collaboration in cross-cultural research. Foremost among these is the type of reinforcements for or against interdisciplinary collaboration they will receive for their efforts. Although the ideal of an interdisciplinary cross-cultural approach to social science questions might be advanced, the royal road to academic adequacy and the institutional rewards that go with it is still one of disciplinary centrality. For example, even when an interdisciplinary committee is used to decide on promotion and tenure, the vote of committee members from the same discipline as the candidate is likely to be weighed more heavily than other committee members' votes. While it is clear that the major career reinforcements, such as promotion and tenure, are to be gained through disciplinary centrality, it is less obvious that many of the day-to-day supports (e.g., reading similar literatures) for continued interdisciplinary efforts are hard to find within a discipline. The subtle and not so subtle negative reinforcements may at times completely extinguish the interdisciplinary response.

The problem of disciplinary marginality is often compounded to an identity-diffusing degree. One is, first of all, marginal as an interdisciplinary within a discipline, then as a cross-cultural research, and then (if a cross-cultural project staff is formed) as a minority member of a multicultural group without a clear majority. The topic, method, and location of the research may all bring additional marginal elements to the process. It should now be clear why, until quite recently, it was primarily the established members (e.g. tenured) of the various social science disciplines who chose to engage in interdisciplinary cross-cultural research. Neophyte members of the trade might advocate the interdisciplinary approach but they would do well to consider the implications for career and personal finances before actually undertaking such endeavors.

Another disciplinary constraint is found in the different levels of conceptualization of research problems and the terminological confusion that arises. For example, the psychologist's use of the term ego and the anthropologist's use of the same term denote quite different things in their respective disciplines. If the terminological obstacles can be overcome (and they are among the most easily overcome of the disciplinary constraints), one is then faced with the integration of disciplinary approaches to similar research problems. The core problem of the validity of each discipline's theories and generalizations can be partially solved on a conceptual level by the recognition that many of the social sciences are approaching the same or related problems at different levels of analysis. Political attitudes, intergroup relations and group-leader interactions are among the topics that anthropology, sociology, political science, and psychology might deal with at different levels of analysis. Psychology with its emphasis on the individual and the individual in relation to the group cannot, of course, deal with the total variance in human behavior and other disciplines that use a sociocultural level of analysis (e.g., kinship systems, collective group behavior) must be part of any composite social science competence.

Unfortunately, the historical accidents that led to the present organization of scientific content into different disciplines has also led to a type of academic ethnocentrism which mitigates against the integration of different levels of analysis of social science phenomena. Each in their own way lays claim to truth or at least a greater share of the truth in explaining human behavior. And human behavior continues to be perversely refractory to explanation by any one level of analysis.

Separating the various social science disciplines into their respective content and method areas appears to have been largely the doing of immature sciences that were evolving by historical accident in response to societally relevant research needs. The history of clinical psychology in the United States, which developed in response to a combination of societal needs spawned by world wars among other things, is a case in point. The current emphasis on educational psychology in response to national needs in many developing countries is another case in point. That many of the social science disciplines were inherently interdisciplinary in the early stages of their origins seems to have been lost in the trend toward disciplinary specialization and specialities within disciplines.

Regardless of how one views the historical origins and subsequent development of the various social science disciplines, the current within-discipline training models are not conductive to the production of interdisciplinary scholars capable of meaningful cross-cultural research. Moreover, in many cases, the various disciplines actively proselytize for the superiority of their methodological approaches. Even the most benign training programs often reinforce disciplinary ethnocentrism. As one wit has observed of his experiences at interdisciplinary research conferences, "seeing the ways other disciplines go about their research always makes me feel good about my own." This only mildly facetious methodological imperialism is clearly visible in the interdisciplinary relations between psychology and anthropology. The emphasis on experimental method in psychology (viewed as applied on trivial content by anthropologists) and ethnographic analysis in anthropology (viewed as lacking in essential research controls by psychologists) depends on the disciplinary perspective. That the internal validity of the psychological experiment might be enhanced by prior ethnographic analysis of the phenomena under consideration is seldom acknowledged, let alone acted upon.

Organizational/Institutional Constraints

The most obvious organizational constraint is the sheer lack of budgetary-promotional professional institutions whose mission is the support and facilitation of interdisciplinary cross-cultural research. There have been examples of successful interdisciplinary social science research organizations, but it is very difficult to find a counterpart in cross-cultural research. The academic institutions of most countries certainly have not been aggressive in instituting such programs and the future does not appear bright.

There is reason for some optimism about the future of interdisciplinary cross-cultural research to be found in private sector institutions that perform research for sponsoring groups, most typically the national government or national foundations. Pressing social problems of various countries, for example, often have their counterparts in other countries and the knowledge needed to solve such problems can best be generated by collaborative research which is more cost-effective in the long run. Moreover, the scope of the problem often requires an interdisciplinary approach to its solution which is additional cause for optimism. Unfortunately, the organization of contributing disciplines and the conceptualization of the research plan is often influenced more by bureaucratic processes than by the demands of the problem.

If one chooses to do interdisciplinary cross-cultural research based on scholarly motives and not financial incentives, the organizational/institutional support often proves much harder to obtain. Without a significant problem focus of some immediate concern to a sponsor, one is often faced with the need to subsidize the research from personal resources or abandon the project altogether.

Management of Interdisciplinary Cross-Cultural Research

Presuming one has conductive organizational/institutional arrangements, and is capable of overcoming the actor factors, myths, and disciplinary constraints and bring together a group of reasonably amiable, problem-oriented realists, one is then faced with formidable research management problems. These problems are fairly simple when dealing with a two-party collaboration but compound quickly when three or more parties are part of the process. At the extreme level of difficulty, it is possible to have interdisciplinary multiethnic teams from different countries studying problems of common importance with indigenous methodologies.

All the problems in cultural misunderstanding that have been identified by intercultural communication research are present in such an enterprise and need not be elaborated here (Samovar & Porter, 1976). Other recently emerging issues are also part of this enterprise and will undoubtedly contribute to the complexity of it. To mention only one, there is an increasing trend toward what has been termed indigenization of the social sciences (Kumar, 1977; Singh, this volume), which refers to the less industrialized countries developing social sciences that reflect their cultural heritage, present needs, and political and economic goals. Taken to their logical extreme, structural, substantive, and theoretical indigenization all would appear to contribute to a lowered probability of interdisciplinary cross-cultural collaboration between culturally different researchers, at least for the near term. That the long term consequences of indigenization of the social sciences may indeed be supportive of the norm of the university of social science and parity between culturally different researchers is another issue. The important point is that for the purposes of present research programs it adds another element of complexity to an already complicated activity.

As others have indicated (e.g., Jacob & Jacob, 1977) the skill of diplomacy in managing interdisciplinary cross-cultural research is a largely untried pattern of human organization requiring the skill of diplomacy. Those who survive the experience to go on to other collaborative research relationships are a clear minority with genuine commitment to work cooperatively in pursuit of the validation of generalizations produced by social science research. The relative scarcity of those willing or able to acquire and practice the competence necessary for continued progress will undoubtedly continue to be a major obstacle.

Potential Solutions

Thus far emphasis has been on obstacles to interdisciplinary collaboration in cross-cultural social science research. This section will deal with some potential solutions to the problems presented by these obstacles. Solitary interdisciplinary approaches, unique interdisciplinary specialization, and coordinated interdisciplinary team efforts will be discussed as the more salient potential solutions.

Solitary Interdisciplinary Approaches

Probably the simplest type of solution to the many obstacles discussed above is for an individual to integrate within one's own work a research topic that is manageable yet significant enough to warrant investigation by a solitary investigator who draws on the theories and methods of two different disciplines. It is important to understand that this approach does not involve the integration of the totality of theories and methods of two different disciplines but rather is focused on the selective integration of theories and methods in a narrow spectrum of two or more different disciplines.

There are many advantages to this approach. For one, the need for coordination between collaborating entities is eliminated and the problems of personal compatibility and communication are minimized. Moreover, the simultaneity of events required for two-person or more collaborative activities is greatly reduced. The solitary interdisciplinarian can simply acquire the expertise in the needed theories and methods of the disciplines that bear on the research goals in the order which is most compatible with current career and personal life style variables. There may be large gaps in the time between the acquisition of knowledge of theories and methods and their application to the focal research problem. The essential point is that the interdisciplinary integration takes place within a single mind, rather than being a product of exchange and integration between two or more individuals. The relationship of cultural variables and psychopathology might be considered one example of the type of research problem for which the solitary interdisciplinary approach would be appropriate (see paper by Higginbotham, this volume.)

The disadvantages and limitations of this approach are also obvious. As discussed above, the limitations on mastery of one disciplinary area are severe and mastery of two is an impossibility. Thus the type of interdisciplinary collaboration advocated by this approach may be quite narrow and has all the limitations that are placed on the time and expertise demands of a single individual. It will work for those research questions that are possible to define within rather narrow limits, but there are very few social science research questions that may be so defined without losing relevance or significance. However, where it is possible to identify and define a research problem that involves selected theories and methods of two different disciplines yet is capable of attack by a single individual, this may be the interdisciplinary approach of choice.

Unique Interdisciplinary Specialization

The essential feature of this approach is the concentration on the overlap of narrow specialties among researchers within the various social science disciplines. According to at least one investigator (Campbell, 1969), this is our best hope for a comprehensive social science that is currently being kept from emergence by the ethnocentrism of the various disciplines. Campbell advocates a collective comprehensiveness through overlapping patterns of unique narrowness, (typically by one researcher) which he describes pictorially as the fish-scale model of omniscience. Extending the approach advocated by Campbell to overlapping narrow specialties among researchers appears to have many advantages for the area of cross-cultural social science research. At this stage of development, both the theoretical and empirical foundations of cross-cultural social science research would certainly be strengthened by data produced by this approach. For example, the analysis of core problems in the social sciences (e.g., the social stimulus situation) which involves attempts to bridge the gap from the laboratory to field research could profit from the differential expertise in different sets of methods employed by the different disciplines, and the refinements of those methods embodied in the individual expertise of various members of those disciplines.

A major limitation of an approach which advocates the overlap of narrow specialties within disciplines is that the substantive problems of cross-cultural research are so formidable and the available specialists are so few that it is questionable that any meaningful progress could be made. Adding the cultural dimension to social science research may have shaken the various disciplines out of their provincialism, but it has also opened up a vast new area for which the person-power is simply not available. Far from the redundant proliferation of highly similar specialties that Campbell views as creating interdisciplinary gaps, cross-cultural research can point to very few specialty areas which have been adequately investigated, let alone definitively studied. Considering its relative youth by comparison with other subdisciplines within the social sciences, it may be a long time before it is reasonable to encourage narrow specialization in cross-cultural interdisciplinary social science research.

Coordinated Interdisciplinary Team Efforts

Potentially the most effective current solution to the tasks which lie ahead for cross-cultural social science research involves expanding the unique interdisciplinary specialization approach into team efforts on problems of mutual significance for the theoretical and empirical advancement of the respective disciplines and the validity of social science generalizations. However, the cultural dimension adds complexity on both the substantive and practical levels of research that must be dealt with in order to achieve meaningful comparative outcomes. One variation might be for interdisciplinary teams within a culture to function relatively independently in producing research which is then shared with interdisciplinary teams from other cultures.

Cross-cultural research employing the interdisciplinary team approach represents considerable potential for producing the type and amount of evidence badly needed by the social sciences. It increases the breadth of research problems which can be undertaken and provides increased opportunity for members of different disciplines to develop more integrated and substantive depth in the problems under investigation. The unique interdisciplinary specializations which provide the building blocks for coordinated interdisciplinary team research should not be mistaken for a narrow or shallow approach to problems of importance for cross-cultural social science research. On the contrary, the problems that can be dealt with through this approach lead to increased collective competence in cross-cultural social science research.

While the potential is great for coordinated interdisciplinary team efforts, it would be well to maintain a cautious optimism about what can realistically be achieved. The considerable investment of time and energy to finance and organize an effort on this scale may prove to be a major disadvantage. The subsequent demands on financial and human resources and the entrepreneurial skills needed to maintain such programs have been identified by Jacob and Jacob (1977). The essential difficulty will very likely be in the area of research management strategies and skills that are effective for handling the many problems that inevitably occur in such undertakings. Social/organizational inventions which foster the needed skills will have to be made and budgetary/promotional institutions will have to be developed and allowed to operate with the flexibility necessary for evolving patterns of effective collaboration.

Toward Interdisciplinary Research Competence

It should be apparent by now that there are pressing needs for research on the cross-cultural interdisciplinary social science research enterprise. Such research should be aimed at developing and enhancing the individual and collective competence of the people involved with this enterprise. The most pressing research needs can be classified along roughly the same lines as those which were used to organize the obstacles and potential solutions described in previous sections, and fall into individual, group, and institutional /organizational categories.

A critical area of research concerns the personal characteristics of the participants in cross-cultural interdisciplinary social science research. However, we should not be misled into placing too much emphasis on these factors as opposed to the situational variance in collaborative activities. More importantly we should be looking for evidence of the person by situation interactions that account for increased effectiveness in cross-cultural interdisciplinary social science research. For example, investigation of the types of leadership strategies that are most effective at different phases of interdisciplinary cross-cultural research conducted by groups of different cultural composition could yield important information for project management. Another much needed study would involve identifying the most probable problematic situations in interdisciplinary cross-cultural research activities and developing broadly acceptable criteria for judging the effectiveness of responses to these problems. While there have been many failures, there also have been exemplary projects (e.g., Sartorius, 1973; Cooper et al., 1972) that can be studied to find the common elements, if they exist, of successful projects. An additional question concerns the design of training programs to equip people with the mediational skills that are critical to the research process, and the generalizability of training in other areas (e.g., politics) of international diplomacy for research management.

Although the human component is undoubtedly a critical element in successful projects, it is also important to study the subjects which yield to the cross-cultural interdisciplinary approach. Far more attention must be paid to the type of research topics that are amenable to this approach and to those which are not, and the theoretical and methodological aspects of the topical limitations. An important research question concerns the effects of cross-cultural interdisciplinary research on the development of the social science disciplines. A far more sanguine attitude toward the potential of cross-cultural interdisciplinary research might emerge from such examination, irrespective of the validity of the claims for increased scientific validity that seem to underpin the whole enterprise.

An important aspect of the future of cross-cultural interdisciplinary research deals with the structural variables that make one or a combination of the potential solutions described above effective for a particular research goal. There is little in the literature that identifies the organizational /institutional arrangements that have proven effective in promoting and enhancing the enterprise, and those that have been effective in the past may not be effective in the future. Equally important is the question of the incentives that are necessary to promote and enhance cross-cultural interdisciplinary research and how these can be provided while maintaining parity of theory, method, and interpretation among the participants of research teams from different nations.

The short-term and long-term effects of the various national indigenization movements in the social sciences will be of particular concern in future cross-cultural interdisciplinary research. Granted that the age of opportunistic cross-cultural research has largely passed, what do we know of the means for deciding on the organization of future research? Parity is the password and there are many researchers who by dint of their academic socialization are poorly equipped to practice their research arts on this basis. Patterns of disciplinary and cultural exchange in social science circles will undoubtedly be enlivened by the inevitable need for collaboration at the same time that new research problems are appearing that challenge the limits of traditional approaches to their solutions.

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ORGANIZING FOR CROSS-CULTURAL RESEARCH

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An understanding of the problems and potentialities of cross-national research requires knowledge of the organizational forms which have made its implementation possible. The interplay between the nature of the research area and the attributes of the organizing component which foster the attainment of the research activities as well as the formal and informal properties of organization are exemplified by specific projects. It can be seen that in mapping out a field of research activity, a variety of roles and identities are assumed; and, in the process, the researchers become both the observer and the observed.

Caplow (1964) has used the term "community" to refer to groupings which show some of the attributes of organizations. Goode (1957) suggests criteria for such communities which have been labeled as structural, normative and motivational. An analysis of these characteristics shows that they permit the structuring and completion of cross-national research along the lines of communication patterns or "networks." These "networks" have evolved as adaptive mechanisms to bridge the gaps created by the artificial boundaries of geography, organizations and institutions and therefore allow for a satisfactory flow of ideas, decisions and information. Different types of "networks" have emerged in the context of varying points of reference. Within them, exchanges occur in the form of forums, as specific task formats such as round-tables or seminars or in the resolution of the kinds of implementation problems upon which the research is expected to focus. In significant ways, these "networks" have included the training and exchanges of personnel. Another kind of "network" which draws heavily from other "area networks" has occurred in view of the need to focus on the socio-economic problems of development in the "developing countries."

The existence of these "networks" among researchers, who otherwise would not be in contact with each other, implies linkages to certain identifiable organizational requirements. These requisites, in turn, imply the ways in which social science needs are expressed. There has, however, been an observed reciprocity in influence between the organizational forms and such variables as the conceptions by the researchers of their occupational roles, the significance of the subject to be investigated and the generation and application of modes of investigation to social science phenomena which occur through these exchanges (Crawford and Biderman, 1969). In the light of these interactions, it would be useful to look into the kinds of orientations that social scientists have.

In view of the new technology, are researchers thinking of the "futures," of the "preferred futures," or do they envision scenarios for the kinds of social analysis which must be at the heart of cross-cultural research? Shall we conceptualize the purpose of organizational efforts as springing from a knowledge of the assumptions of cross-cultural research? Have these assumptions remained sometimes unidentified and unexpressed? Is the goal of cross-cultural research primarily to increase knowledge of other cultures? If it were an exercise of methodological rigor, how would it come to grips with the accompanying problems of comparability due to the phenomenon of between-nation variation? What kinds of machinery would lend best to the construction of interchangeable measures and the need to cope with the requirements of uniform data collection and testing procedures across national boundaries? It has been implied, in some of the selected projects reviewed in this paper, that the organizing principles might be essentially the same as those which occur in national studies. Another issue which needs clarification is whether cross-cultural research findings will be relevant to the development thrusts of the Third World countries. Development-oriented research is Western, an American phenomenon which is deliberate, planned and organized. The existence of an adequate core of resident scholars who are skilled in both basic and applied research would provide research programs with a self-sustaining feature. Also, it has been said that as long as research in a Southeast Asian country is cumulative in a certain problem area it does not really matter whether the research is done wholly by native scholars, by resident foreign colleagues, or by a collaboration between the two (Corpuz, 1968). From this perspective, a cross-cultural relationship in support of developmental efforts promises to become an exciting relationship.

Defining and Operationalizing Cross-Cultural Research in the Third World

Third World social scientists have been defined on the "outside" as having many role dilemmas in terms of their institutional affiliations and their perceptions of their relationships to the power structure (Carino, 1977). Considerable changes in the general intellectual climate from which social scientists draw their sustenance have brought additional shifts in their perspectives and changes in coping responses. Soedjatmoko's (1968) description of social scientists in the contemporary situation is most apt, for he sees them as emancipated from culture-bound notions that modernization is inextricably linked with Western models. "Less erudite, less cosmopolitan, and imbued with a greater self-confidence" due to the "increase in their numbers who have training in the social sciences [and] a growing domestic audience," they have "less concern for outside approval for their intellectual activities, but they show a considerable interest and faith in the social sciences, especially in what the social sciences can do for the modernization of their country (Soedjatmoko, 1969-70, p. 12)." There are two main items on the agenda for the Third World scientist. First, as Soedjatmoko puts it, "there could be no greater danger for the young social scientists . . . than to lose themselves in the kind of social research which is a mere extension of the traditional academic concerns in the stable countries of the West, and to remain blind to the more basic issues that will have to be identified and defined by the intellectuals of the Third World themselves" (Soedjatmoko, 1969-70, p. 13). Second, in the context of the framework for the orientation of in-country research, the step of problem definition is most crucial, since the problem as formulated should reflect national priorities and goals. Intellectual challenges for the social scientist stem from a variety of sources. They are inherent in the kinds of concepts he has of the societal transformation that would be demanded, in the processes of relating value patterns to the changing realities in the search for alternatives, and in the values he holds when assessing the significance of each new development in the light of common goals. The social scientist's most important role may be seen as one wherein he will have the flexibility to operate on the national level, in a communal framework and transcommunal relationships.

The social scientist has another role, that of a linkage agent with a rapidly changing world which is in crisis. The national capacity to understand these world-wide changes and the ability to provide a direction which is in line with the interests of a people are attributed to the quality and breadth of its social scientists. The perspectives which the social scientist distills from different modes of living are elements which are envisioned as significant forces which shape the conception of the kind of life he wishes his country to have. Individual freedom, the strength of the intellectual institutions, and the prevailing standards and criteria of performance in these institutions are important considerations in seeking answers to issues brought about by politics, on the one hand, and human conditions, on the other. These elements are pertinent to understanding the "inside view." The people who are involved in the process of development see very little relationship between the traditional preoccupations of the research effort and developmental problems. The best assurance for attaining a meaningful cross-cultural research relationship is to develop the research capacity of the Third World scientists, their facilities and their staffs and to hopefully build on their desires to make significant contributions to our knowledge of man and what man hopes to be. Therefore, it is pertinent, at this point, to look into the emerging patterns of American research efforts in Asia as well as the patterns of organization of research structures in the same region. Suggestions should be culled as to how Asians think regional cooperation may be institutionalized.

Operationalizing American Research Efforts

Along with the emerging *Zeitgeist* regarding developmental efforts in Asia and the awareness of the need for international cooperation within the social sciences, there have been striking increases in the number of scholars and researchers who are operating in direct contact with other nations and who are assigned for varying periods of time to work in foreign settings in collaboration with members of other nations. Multilateral and bilateral agreements, institutional arrangements and organizational supports spawn a new breed of interaction processes, where participants enacting supra-national or non-national roles represent a new kind of international society. These research relationships are an added dimension of cross-cultural contact. In this part of the paper, we will be primarily concerned with the approaches that have been used to design and operationalize a research program within the region.

Castillo (1968) presents a typology of the scope of relationships that take place for the operationalizing of American research efforts in Southeast Asia. At one level the person-to-person relationship is manifested in the *modus operandi* of the independent researcher. At another level, it is expressed in the cooperative arrangement between an American professor and a local professor in a joint research project in which each investigator has equal access to data and publication may occur in both countries. Interinstitutional arrangements are another form of cooperation which may combine the components of teaching and research or research alone. In addition, there may be a training feature for the local personnel in-country or at an alternative site in the United States. American graduate students are likewise beneficiaries of these research arrangements. The bonds of cooperation forged at each of these levels are necessary and irreplaceable. A new element in American foreign policy sees these exchange relationships as a mechanism for developing mutual dependencies internationally among intranational institutions. These dependencies involve educational institutions as well as other governmental agencies and the aim is both educational development and the promotion of peace and understanding (Frankel, 1968). They are, however, temporary answers to the intellectual concerns of scholarship, the generation of research concepts and the analysis of research findings. The manner in which Third World scholars and universities work on these short-term, oftentimes fragmented projects is not only seen as expedient in character but also as a hindrance to long-term commitments. How a sensible balance can be effected in these exchange relationships still remains to be answered. The "brain drain" phenomenon is a concrete manifestation of this imbalance to which Third World countries have addressed themselves with vigor. Extensive, drastic revisions of the curriculum at the tertiary level and study service schemes are other examples of national attempts to redress this imbalance.

Another version of an organizational strategy is that which occurs when the prestige of an American professor enables his Southeast Asian graduate student to embark quickly on a research career in his home country. In still other projects, both the professor and the American graduate student may be hosted in the Southeast Asian country. The visiting professor, who has an appointment in a local university, is part of the local institutional research efforts. Other Americans who are on the staff of local institutions or of international agencies may become invaluable contacts for some of the personal arrangements among American nationals. These schemes demonstrate that some projects can be essentially individual undertakings. Moreover, collaborative projects, which, in principle, have clear advantages and need to be undertaken whenever possible, might be neither desirable nor feasible.

The problem of saturation by researchers and the development of better local relationships and responsibilities to the host country, its social scientists and the subjects of the study imply that some kind of *quid pro quo* is involved. Recognition of the deficiencies of the prevailing state of affairs prompted professional organizations such as the American Psychological Association, the Society for Applied Anthropology and the American Sociological Association to adopt a professional code of ethics to sensitize individuals to problems created by individual irresponsibility. The hope has been expressed that the majority of social scientists would agree on a basic code of professional conduct. Violations might then incur sanctions already extant in the academic and scholarly world rather than those associated with some new enforcement machinery (Beals, 1969). However, recent developments in in-country policies regarding the research function make these problems academic. In applying for an entry visa, the social scientist will need to accept responsibilities toward the government of the host country and will be expected to abide by guidelines regarding the purposes of his research and to subscribe to the principle of full disclosure. There are local regulations with which to contend. One such regulation requires that local staff for the data gathering phase need to be approved by duly designated offices.

Institutionalizing Controls Regarding Cross-National Research Efforts

Where the source of funding has been largely foreign and the research directors, expatriates, one of the early mechanisms of control used by a locally-based research institute was a policy which provided that the research activities and the public behavior of project directors were subject to review of a Committee, the majority of whom where locals (cf., Castillo, 1968). Another strategy has been to establish country study teams in several nations. In concert with academic institutions, these teams planned and implemented the research to insure both the quality of the research and the translation of research findings into meaningful policy recommendations. An example of this approach was the cooperative regional project (IDRC, 1972) among three participating countries: Indonesia, Thailand and the Philippines. In two countries, Indonesia and the Philippines, responsibility for the project rested with a country's Coordinating Committee, composed of senior researchers from the participating institutions. For Indonesia, a full-time staff supported the Committee. The Philippines organized technical representatives of the participating agencies into a Technical Working Group, directly under the Committee, and designated a head for this group who in turn directed a full-time staff of researchers. For Thailand, members of the project were associated with a private, non-profit organization affiliated with the national Social Science Association of the country. The organizational features of the project had several components. A Coordinating Committee within this organization was set up for the project. Full-time researchers complemented this country task force. Meetings were scheduled at regular intervals in various locations in all the participating countries. A final meeting was also scheduled. Senior researchers evolved the implementing details of the research design. Budget categories for each country included staffing, travel, data processing, administrative costs, publication and contingencies. Conferences and meetings were administered by the sponsoring agency. It is clear that from the conceptualization of the study down to the minutest details of the monitoring system, the country study teams were in complete control of the research process. The project also allowed for variability in the structuring of the administrative machinery—the Coordinating Committee. The sponsoring agency made it possible to experiment with this approach.

Thus, the cause of cross-national research is furthered: (1) if the boundaries between the sponsor and the researcher are blurred, (2) if the focus of the research process goes beyond the boundaries of concerns which are limited to one country, (3) if it is cooperative and countries come together as equals in the design of the research and in the translation of the findings into policy recommendations and (4) if the problem defined is reflective of the needs of the countries involved. The continuing challenge would be in the participatory process of evolving a relevant methodology and *not* in its transfer. Meshing intra-country expertise with inter-regional and international perspectives in the project design are considerations more important than tradition. Establishing links with a national social science sub-culture could productively contribute toward both international goodwill and the development of an international social science.

Patterns of Growth of Organizations for Educational Research in Asia

Having explored relevant dimensions in relation to the schemes for implementing research in some countries, I include in this section of the paper a discussion of growth patterns of organizations for educational research and development in Asia (see Appendix A). Organization for research and development has occurred under government sponsorship primarily with variations in the scope of the functions. Of late, specific structures are identified with curriculum, methods, materials and facilities (see Pattern II, Appendix A). Autonomous bodies (councils, institutes, committees) and other mechanisms which bring in research expertise (outside of ministerial personnel) are emerging features of organization which could be attributed to the foreign aid for research identified elsewhere in the same report for three countries—Republic of Korea, Philippines and Laos (cf. UNESCO-NIER Regional Programme for Educational Research in Asia, 1973).

Organizing for Cooperation and Collaboration

It would be highly desirable to provide visibility for collaborative programs not only because it is the most logical vehicle for international understanding but because of the very nature of cross-cultural research (see the paper by Dinges, this volume). Some important considerations would have to be examined. International assistance might be directed to the training of personnel, both methodological and managerial, coordination of efforts at the national level and providing substantial back-stopping of researchers with documentation, translation, publication, library and other technical services (such as exchange services so that the outcomes of on-going research in the various disciplines, research institutes and centers could be disseminated more widely). Cost-sharing and follow-up programs could be built into these cooperative activities. Annual conferences of participants in these collaborative projects would serve as motivational and standard-setting mechanisms.

The following section of this paper presents examples of collaborative research projects.

Patterns of International Cooperation Research

Capsule discussions of specific projects reveal variations in the approaches of rationalizing the research efforts. Each investigation brings with it some significant meaning for future development. The International Studies of Values in Politics and the International Evaluation of Achievement (IEA) studies both were initiated by International Councils or a Committee and implemented through a national centers network. The IUSSP project was essentially a personal network model initiated by a specialized research institute.

The Values and Active Community Model

The International Studies of Values in Politics (1971) provides a prototype of a pattern of coping with the manifestations and analysis of social science phenomena. Joint cross-national teamwork in the design and day-to-day conduct of the research started with the extension of sponsorship to the Project by the Council. Specific working agreements with cooperating institutes in four countries (India, Poland, Yugoslavia, and the United States) provided that

(1) the research design would be jointly determined by the principal participants from all four countries;
(2) each national team would be mainly responsible for carrying out its own field work;
(3) analysis and evaluation of pooled data would be assigned to cross-national groups working face-to-face on specific phases of the program; and
(4) conclusions would be submitted to successive round-tables, where progress would be checked and further guidelines agreed upon.

A liaison committee of the Council acted in an advisory capacity for the project and one or more of its members served in the Secretariat of the round-tables. Each country had the task of locating interested appropriate scholars and qualified locally responsible institutions and support, particularly financial support from the governments. In view of the different scientific climates of the countries involved, the participants "learned not to demand complete identity of purpose as a condition of establishing common operational procedures and a common program of analysis. It is essential to have a high degree of respect for other people's way of thinking. For this kind of collaboration, some personality types are more appropriate than others." (International Studies of Values in Politics, 1971, p. XVIII)

One of the methodological perspectives gained from this project is the significance of the contexts where the observations were made, for they provided the "meaningfulness" and the "interaction" of specific variables. In this four-year, four-nation project an identical methodology was implemented in parallel studies; it consisted of a leadership survey, an inventory of community activity, identification of the local level of economic development, a probe into the local political decision-making processes and value content analysis of leader statements. The method of controlled comparisons made it possible to document the differences and similarities across the four countries. Groenman (1971) summarizes the pay-offs from the project as the generation of new ideas, hypotheses and instruments, changes in perspective, a control over too easily formulated generalizations, remedies of parochialism or national compartmentalization, a better understanding of the different theoretical approaches in social science, and the creation of a truly international social science community. This is understandable in view of the format in which the project evolved. Periods of testing were interspersed with round-tables held in and rotated among the countries at periodic intervals, thus ensuring the involvement of junior support staff with supplements of personnel exchanges; these were complemented at the principal participant level by long periods of visitation. A multinational editorial committee guided the completion of the report. Other reports are forthcoming from the intensive interchange that occurred in the project. The cost-shared program of research demonstrated the mesh in the resources of both the public and private sectors in the form of contracts, grants, institutional facilities and personnel.

The IUSSP Project (International Union for the Scientific Study of Population)

This one-year project used as sources materials from a number of earlier fertility surveys carried out individually on two substantive issues: the sex preference for children and the correlates of "wanting no more children but not practicing contraception." Fredman and Coombs (1974) of the University of Michigan Population Studies Center organized and conducted this project largely by mail. Initial contacts were made with a number of known Asian investigators; European and American investigators were added to provide the contrasts between developed and developing areas. Despite the differences in questionnaires, concepts, modes and standards of field work, coding and table formats, the output was useful as a vehicle for the in-depth discussions of common problems by the 17 investigators. These discussions were quite useful in pointing out a number of methodological problems associated with survey data. Considerable correspondence and more face-to-face conferences or the central tabulation of data were implied as alternatives to solve the problem of achieving detailed comparability of data. The unwillingness of some countries to release data for tabulation elsewhere precluded the use of the latter alternative. SIDA (Swedish Development Authority) underwrote the cost of the Seminar Conference in Brussels, the ensuing secretarial work and the duplication of manuscripts and tables. Individual investigators contributed most of the work in the project.

The IEA Model (International Evaluation of Achievement)

The need to introduce into comparative educational studies established procedures of research and quantitative assessment was articulated by Anderson, Foshay, and the Eltham group of researchers who had participated at UNESCO meetings. For the initial inception of the project, researchers representing national centers constituted themselves into a Council for the International Evaluation of Educational Achievement (IEA) at a meeting in October 1960 at the UNESCO Institute for Education in Hamburg. Five important administrative decisions were made at this level:

1. Additional members from other research institutions could be coopted into the Council.
2. A distinction was made in the kind of membership in the Council. Those who participate fully in the test construction phase would have full memberships. The associate membership category would be extended to those who join later but who would administer the tools developed by the full members.
3. A standing committee which was appointed by the Council appointed a chairman who would have overall supervision of the project.
4. UNESCO Institute for Education, as Coordinating Center, was to provide initial administrative and technical coordination and attach to its professional staff a coordinator within the framework of Council and Standing Committee decisions who would have executive responsibility for the scientific and administrative organization of the project.
5. The creation of the post of Technical Director.

The role of the Council was envisioned as a kind of "parliament" which would meet yearly for policy decision. Topics suggested for deliberation included substantive areas of investigation, the nature of target populations, proposed timetables and prospective financial arrangments. The standing Committee implemented policy decisions, made necessary interim decisions between council meetings, appointed consultants and made arrangements for the write-ups of the report.

Despite limitations in sampling and test construction, a pilot study in 12 countries demonstrated that it was possible and meaningful to carry out the project as planned. It also showed that the project would be useful in generating information for the development of hypotheses.

As a rule, council meetings were one-week sessions held in Hamburg. During these sessions, work was used as a format to deal with the methodological requirements of test and questionnaire construction, sampling design and hypothesis formulation. Enlarged standing committee meetings included test constructors, questionnaire constructors and the international sampling referee. Special meetings were held by the Test Construction Committee, the Mathematics Teaching experts, and international consultants in the fields of test construction, statistics, data processing, sampling, and the mathematics curriculum. At the coding and punching stage, meetings were also held among members of the IEA and visits were made to some national centers. The communication system also included a synthesis of project decisions, procedures, and plans in bulletin form (four were published) as well as individual and circular letters.

At the national level, research institute participation was formalized by designating national centers for research. Sampling experts and case-study informants were designated for each country. Selected experts in each country assembled materials on instructional specifications for the target age groups. International definitions of target populations were formulated. Sampling proposals for each country were finalized in correspondence with the international sampling referee. To standardize the test administration phase, three manuals were prepared, each designed for specific target groups, e.g., the centers, the school testing organizers, and the test adminstrators. Elaborate check procedures were devised for each center level to insure that the international coding and punching scheme was followed. A data bank and a data bank manual were designed for qualified users. Reports on the results of the tests were sent to each participating school. Finally a flow chart was developed to show the sequence of activities necessary for completion of the computer analysis for each hypothesis.

What are some of the organizational insights generated from a fairly large-scale project of this kind? Husen's IEA group (Husen, 1967) has specific recommendations on the design of the components of the organization. They recommend that IEA have a legal identity so that it can receive grants and hire its own research staff. This particular feature would emphasize the group's formal responsibility for the research effort rather than focus on the efforts of one individual council member.

Although guidelines are set for the participating institutes (cf., Husen, 1967), care needs to be taken that the research effort is not limited to only those nations which have financial resources in the social sciences. Forms of assistance should be made available in order to encourage the participation of less affluent centers. Aside from financial arrangements, there are other aspects of the research endeavor which need examination. A member of the professional staff should be fluent in the language in which the administration of the project is carried out. For projects of long duration, continuity in the administrative hierarchy should be ensured. This could be done if the head of the project were in constant contact with the various aspects of the research, and a full-time junior member of the staff could be responsible for day-to-day operations. This junior staff member should have knowledge of psychometrics and large-scale surveys and should be available for briefings during the planning stage, long before the initiation of field work. Also, in order to accommodate an increased workload anticipated for forthcoming projects, the creation of additional associate centers might be considered.

The coordinating center is conceptualized as the site of briefing sessions between junior research officers from each of the national centers and the technical director, the coordinator, and the international sampling referee. A data processing expert should be associated with the coordinating center early in the project in order to provide information regarding the design of instruments, alternative forms of data analysis, procedures for the collection of relevant data and the appropriate computer-accessible form of information storage. Committees of experts could focus on tasks which deal with the theoretical framework, the definition of the target population, subject matter analysis, and test and questionnaire construction. Finally, the fieldwork stages should be improved so that they include attention to the development of tools, pretesting, sampling, coding, punching, and computation sequences.

The IEA Study of Literature (a nine-country study)

The IEA Study of Literature is the second phase of the IEA studies. In 1964, Foshay (see Purves, 1973) of Teachers College, proposed that IEA would be a responsive vehicle for exploring relationships among a number of variables—language, rhetoric, literary response, and epistemology of educational outcomes. Subsequent research substantiated his idea. At the same time, a group in Belgium proposed studies in the area of mother tongue learning, and Robert Thorndike suggested a multinational investigation of reading. Foshay and Shafer at Teachers College and Purves (see Purves, 1973), then of Barnard College, made plans for another study. For two years they investigated expressed responses to literature. They then worked on the development of instruments which would describe the responses of larger numbers of students. Two years later the first International Committee of five members was formed. At the first meeting, a research design was formulated to explicate the rationale, assumptions, and hypotheses; a grid was devised to describe the intersections of content areas and behaviors. By using the National Center network, each center was subsequently asked to appoint its own Literature Committee. The latter were to be responsible for the following tasks:

1. review the grid of goals, rationales and hypotheses;
2. indicate quantitatively the relative importance of each of the cells in the grid as set forth either in a national curriculum guide or in a consensus of regional or local guides; and
3. submit copies of relevant information such as official guides for instruction in literature, national or regional examinations in literature, titles of widely used textbooks and titles of books on pedagogy.

At the second meeting of the International Committee a year later, the committee reviewed the national reports. They then revised the grid of goals, rationales, and hypotheses in accordance with the feedback they had received from the centers. A cross-over study of the problems of cross-national testing in poetry was designed and conducted on four separate samples of American and English subjects. The Committee requested suggestions regarding these pre-test measures from the various centers; actual pre-testing was completed in 1968. Accuracy of test item translations, selections of test items and the interchangeability of items for both countries in the attitude scales was ensured in three ways. First, at the IEA level, the Committee level, and the Centers, literary translators checked all the translations that had been made for the selections. Second, back translations into English were made for all three measures. Finally, data analyses, sampling design and the application of statistical measures were performed by a team of consultants. This last phase benefited immensely from an earlier study on the testing of achievement in mathematics.

The project received funds from two sources. The costs at the national level were raised within the country while the international costs were raised by the IEA.

In summary, the approaches cited above rely heavily not only on local resources and research but also on the participation of national social scientists, centers and institutions in the formulation and implementation of methodological strategies. The work of organizations which initiate investigative projects presents substantial evidence that research activity can be a valuable mechanism to stimulate needed change, to discover new instruments and procedures, and to negotiate for a climate of exchange and cooperation. It cannot be overemphasized that a research program with cross-cultural features calls for an element of "planfulness" to ensure the involvement and collaboration of the countries' efforts themselves. Another necessary component of cross-cultural research is "a strategy of emergent adjustment" (Shriver, 1967), characterized by flexibility, watchfulness and constructiveness. The organization of research, the receptivity of the research staff to foreign expertise and technical assistance, and the availability of mechanisms for the application of research findings to national decision making are all particularly relevant to the promise of cross-cultural research activity.

Action Components in Organizing for Cross-Cultural Research

One final question might be asked: What is the desired end result of the program? A major general objective should be to clearly delineate suggested area research packages and methodologies at the micro-level which would be applicable to a wide range of situations in both developed and developing countries. The program would assume that there has been an identification, collation, and evaluation of available knowledge and experience in the social science area which would lend itself to cross-cultural experience. Further, it would assume that this knowledge and experience would be disseminated for the benefit of the social science community. The program that would be carefully developed, for a particular suggested area could be of practical value in formulating a national program. It would also serve as a comparative frame of reference for the coordinated micro-level country studies and contribute to the formulation of a cross-national strategy—one more wide-ranging than those encompassed in currently completed studies. Discussions could be generated on the feasibility of joint projects or regional projects. Organization for work and grouping of studies are other considerations which would lend themselves to further exploration as part of the action-oriented strategy.

Appendix A Organizational Patterns

Pattern I: A significant research unit is located within the Ministry of Education and appears to fulfill a function in the mechanism of decision making on educational orientation and planning. This is the oldest pattern, though it may be reestablished in different ways from time to time (examples: Iran, Nepal, Malaysia).

Pattern II: This is a variant of Pattern I. The difference is in the vastly expanded functions of the research unit, which is now not only involved in planning, but also in problems of management and of educational content and quality (examples: Indonesia, Laos, Singapore).

Pattern III: As was the case in Patterns I and II, the Government is the main sponsor under Pattern III. The diversification which appeared in Pattern II is so extended that the functions of planning and content-control are becoming separated. In Pattern III, the Government establishes separate institutions for R and D on curriculum, methods, materials, and facilities. This pattern is quickly spreading (examples: Rep. of Korea, Sri Lanka, Thailand, Rep. of Vietnam, Pakistan, Philippines, and again Nepal, Iran and Malaysia).

Pattern IV: The Government creates autonomous bodies (councils, institutes, committees) entrusted with the functions of R and D parallel to yet supportive of the administrative machinery of the Ministry of Education (examples: India, Philippines).

Pattern V: This pattern functions in conjunction with other patterns. It shows certain mechanisms for the enhancement of "free enterprise" in R and D, with some Government support through grants or through the farming out of R and D projects. The pattern may exist either to boost the quality and involvement of research in non-ministerial and even private institutions; or as a means to tap resources of research expertise for educational development which the government cannot recruit from its ministerial personnel (examples: India, Philippines, Japan, Indonesia, Afghanistan, Nepal).

N.B.—Where Pattern V coincides with Patterns II or III, a hybrid emerges, showing the government recruiting its R and D staff from among the universities and other learned bodies.

Appendix B

IEA Criteria in the Choice of Participating Institutes' Willingness and ability to meet the following:

1. Be able to produce national representative samples;
2. Be able to ensure the cooperation of the schools selected;
3. Have enough resources to carry out large surveys;
4. Have competent statisticians and psychometricians in their service or have access to them;
5. Be able to guarantee continuity of interest and participation in the project; and
6. Be able to secure national funds for the national costs of the project.

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LIMITATIONS OF ANTHROPOLOGICAL FIELD WORK[1]

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Any discussion of methodology in anthropology must set out clearly how the project has been formulated, how approaches and techniques are modified and how frequently theoretical orientations are altered, amalgamated and rearranged to fit the actual situation in the field. Such a discussion must also define the researcher's position in the field, his limitations, the limitations of this techniques and tools; in fact all those factors which may influence the range of materials made available to him and possibly bearing on the analysis of his data. In this paper I will focus on these latter influences. Too often these extraneous, often emotional effects on the data collection process are swept under the carpet in our attempts to present a holistic, well integrated picture of the local community in which we work and invest a good deal of our intellectual and emotional resources.

Such an enterprise is particularly important in view of the fact that ethnographies form the basis of much comparative anthropological research (Whiting, 1963) as seen in the studies based on the Human Relations Area File, the work of Whiting, *et al*, in the Six Culture Study and publications from Murdock's cross-cultural cumulative coding entre (e.g., Murdock and Wilson, 1972). Starr and Wilson aptly support this notion in a companion paper in this volume where they argue that ethnography is probably the primary and most basic level of research in a cross-cultural context. Ethnography then, it may be argued, is essential to cross-cultural research, and "good ethnography" is based on good participant observation, one of the "traditional" anthropological tools.

I have worked among Aboriginal[2] people in "Rural Town", south-west Queensland, Australia during 1974 and 1975. Since this time I have made numerous return visits to the area. This period of field work constitutes my second major field work effort. Previous research during 1969 through to 1971 was carried out among urban Aboriginal groups in south-east Queensland. A number of these groups have kin-ties with the Aboriginal community in Rural Town. Consequently I have had some contacts with the Rural Town community since late 1969.

Throughout my field research I have made us of "traditional" anthropological methods such as participant observation as well as less traditional methods such as surveys and structured interviews. In this discussion I will focus on participant observation, prolonged discussion with people in crisis and the use of key informants as well as Aboriginal assistants with particular reference to the Rural Town research project. I believe that the extraneous influences affecting field research are most particularly highlighted in these methods and consequently highly relevant in cross-cultural research.

Most discussions in methodology argue the virtue of one particular method, or series of methods, over another. It is not my intention to "defend" my techniques of data collection in this manner. They arose out of the field situation, my theoretical orientation and personal inclination. My task is not to establish that my methods are the only effective ones, but rather to discuss to what extent the data collected by means of these techniques may be accepted as reliable, accurate and precise.

Participant Observation

Participant observation is probably a basic tool in any science and particularly the social sciences. It has been employed by ethnographers for many centuries (see Koepping, 1973). Malinowski (1961) considers it largely a technique by which the field-worker totally immerses himself in the lives of the people he studies and lives with; this implies more than simply "participating" and "observing" (see McCall and Simmons, 1969).

Participant observation is a combination of methods and techniques. It involves genuine social interaction in the field with the subjects of the study, direct observation of relevant events, some formal and a great deal of informal interviewing, some systematic counting, some collection of documents and artifacts, as well as open-endedness in the direction the study takes. As Zelditch (1969) points out, participant observation is not so much a single method as a type of research enterprise, a style of combining several methods towards a particular end, this end being an analytic description of the situation which will aid in the process of Verstehen.

The main characteristics and strengths of this research enterprise are, that it is non-standardized and that it makes effective use of the relationships the field worker establishes in the field in order to refine and deepen the kinds of data elicited. (See the work of Dean, Eichhorn and Dean (1969: 22-23), Pelto (1970) and Jongmans and Gutkind (1967) for example).

But the very strengths of this enterprise are also its weaknesses. The field worker himself is his own principal research instrument, subject to biases, distortions, misunderstandings. As Golde (1970:78), considering her own field work, points out that if the achievement of understanding is the goal of field work, then it is partially accomplished through the field worker's attempts to cope with her own feelings, misapprehensions, impotence, and, at times, sheer stupidity.

Further, the principles of participant observation require the ethnographer to elicit information of a non-standardized form from other individuals (informants) who may also be influenced by biases, distortions, anxieties and misapprehensions. The very fact that the whole data collection process then, is based on inter-personal relationships, means that:

> "the search for validity and reliability is in some way of course as vain as the one for 'grand theories', first because the point of departure, his interest, is for every researcher quite different, and furthermore the form of internalization is very much dependent upon the personality of each different anthropologist as to what he internalizes as well as *how much* he does this . . . Still, if we want to play by the rules of the 'scientific game', we could only assert that the qualitative form of participant observation guarantees to a certain extent the validity and reliability of the findings of *individual* researchers." (Koepping, 1973: 49)

A similar view is expressed by Zelditch (1969) and den Hollander (1967), who also argue against measurements of validity and reliability. Instead, Zelditch (1969:9) maintains, we should focus on criteria according to which the "goodness" of the procedure may be assessed. These criteria include information adequacy consisting of accuracy, precision and completeness of data, and efficiency. These criteria can only be fulfilled if aspects which may influence and limit the data collection process are taken into account, and if not eliminated, at least discussed as valid factors influencing the final analytical description. If the data collection process consists of interpersonal relationships, then "contamination" of data can occur in three major forms: a) the researcher, b) the informants and c) the interaction of researcher and informant.

The Researcher

The researcher is an individual; as such he has varying powers of perception, observation and sensitivity. He is also a product of his own cultural traditions, and social class, his training, his philosophical orientation and academic/economic aspirations. Anthropologists are becoming more and more aware that these aspects which help to shape the researcher's personality must inevitably contribute or detract from his ethnographic abilities. Thus Mead (1973: 249) warns the field worker that "he himself, his own stability and well-being, are inextricably part of the observations which he is making."

Maintaining compatible role-sets. No field worker can be a perfect instrument. While he is in the field, however, he is supposed to reconcile his personal and cultural orientations with the role of the researcher, the social scientist. As Gold (1969:31) points out:

> "Every field work role is at once a social interaction device for securing information for scientific purpose and a set of behaviors in which an observer's self is involved. While playing a field work role and attempting to take the role of an informant, the field observer often attempts to master hitherto strange and only generally understood universes of discourse relating to many attitudes and behaviors. He continually introspects, raising endless questions about the informant and the developing field relationship, with a view of playing the field work role as successfully as possible. A sociological assumption here is that the more successful the field worker is in playing his role, the more successfully he must be in taking the informant's role. Success in both role-taking and role-playing requires success in blending the demands of self-expression and self-integrity with the demands of role ..."

Although the text books discuss at length the principles of assuming different roles, subjecting the self to the research role and so on, there are very few guidelines as to how this should be done. I developed the following strategy.

Firstly, I clearly and openly identified myself as a researcher—an anthropologist who was interested in Aboriginal people in the west, who wanted to find out how they lived, what problems they had, what they remembered of the past, how they coped with the present, how they brought up their children and so on, in fact everything relating specifically to Aborigines in western Queensland as well as all I could find out about life in the west generally. I further stressed that I needed people's help in understanding all these things. I thought I had defined my purpose fairly well until, after three months field work, I discovered that some members of the Rural Town Aboriginal group, thought that I was a social worker. Consequently, they felt somewhat threatened by my presence, in case I would report irregularities in their family situation or social security benefits to the relevant departments. In their estimation this was the 'role' of the social worker. Although I may never have fully communicated to the people what anthropology actually is, I was nevertheless able to dispel some of their fears concerning my role. This became very clear in the patterns which emerged whenever I was introduced to an Aborigine from out of town. Invariably I was presented as: "Anne, my (our) friend (or, Anne, she helps Aborigines, or Anne, she wants to find out about the Mardi's[3]). You don't want to worry about her (or you'll want to talk to her) she's my (our) mate."[4] A really great asset in establishing this "identity" was the fact that I had worked in the area before and had corresponded with two of the families over the intervening years. Certainly I never assigned any role to myself, except that of an interested person who wanted to find out everything. I observed and participated during the first six months of my field work—I was always there, I took part in all activities that were open to me, and I tried, as much as possible, to remain neutral, non-judgemental yet supportive and approachable.

I found, that very soon, people assigned roles to me—the role of friend, of classificatory daughter, of older sister, of helper, taxi driver, photographer, confidant, intermediary in dealings with authority, god-mother, even "banker" to individuals who asked me to look after their money lest they spent it or lost it while on a celebration. I would like to reiterate that these roles were *assigned* to me by the minority group members on the basis of their perception of the relationship we had established. As the content of the relationship became more intimate, so each individual's expectations of the role, in which they perceived me, changed. Obviously my own behaviour adapted accordingly.

At all times, I attempted to balance individual's expectations of me with my expectations of them as informants; that is, I always reasserted the role of researcher so that no matter how intimate the relationship became, neither I nor the people involved, ever forgot that I was there to find out about their lives. I did this consciously for two reasons—ethically it is important that the researcher never becomes an unidentified interviewer, privileged to many confidences which he will use as data in his research without the people's knowledge; methodologically it is equally important that the researcher keeps reminding himself of his task in order to avoid over-identification, or "going-native". (see Pelto, 1970; Henry and Saberwal, 1969)

Reaffirmation of the research role, then, should ensure a measure of rationality. Miller (1969:88) also warns against "over-rapport". He sees the danger not so much in a loss of rationality or objectivity, but maintains that "friendship in the field may seriously limit the researcher's ability to move from one group to another because it aligns the field worker with one particular individual or group and thus precludes entry into another group. Miller further argues that "friendship" restricts the kinds of interaction and questioning open to the researcher. He maintains that as "friendship connotes an all-accepting attitude, to probe beneath the surface of long-believed values would break the friend-to-friend relationship."

I cannot agree with Miller. Alignment can be avoided, at least partially by carefully considering the group's structure before becoming too closely involved with any one group. Further, it is generally possible to be "aligned" with more than one group simultaneously. An "all-accepting", non-judgemental attitude, seems to me crucial. It may not always be possible, but it should be an important aspect of the research enterprise. I personally would not have been able to sustain a relationship which lacked some measure of tolerance and friendship. Perhaps the ideal position is represented by Gold (1969: 35) when he points out:

> "Although the field worker in the participant-as-observer role strives to bring his relationship with the informant to the point of friendship, to the point of intimate form, it behooves him to retain sufficient elements of 'the stranger' to avoid actually reaching intimate form. Simmel's distinction between intimate content and intimate form contains an implicit warning that the latter is inimical to field observation. When content of interaction is intimate, secrets may be shared without either of the interacters feeling compelled to maintain the relationship for more than a short time."

The influence of stress. Relationships as advocated by Gold (see above) can be very stressful to the field worker. While a friend he must remain a stranger, while trying to affirm the role of researcher, he must fulfil the roles ascribed to him by the people and their associated expectations. While striving for acceptance and understanding he must be mindful of over-identification and objectivity. In my case, tensions were also generated by a whole range of personal idiosyncracies: my own insecurities, my desire to please and be recognized as a worthwhile person, my fear of rejection, fears that I would not be able to complete the work in the required time, the realization that not everyone was obliged to like me and by some guilt feelings when I really felt I did not like everyone either.

Every field worker experiences tensions of some kind, this is well documented in Golde's (1970) collection *Women in the Field*, by Pelto (1970), by Jongmans and Gutkind (1967), Henry and Saberwal (1969) as well as McCall and Simmons (1969). As Kobben (1967: 46) points out:

> "Since an ethnographer studies people and not insects, his fieldwork also causes emotions in himself. Personally I lived under great psychological stress and felt little of the proverbial peacefulness of 'country life'. Few books touch on this subject, but I know that the same is true of quite a number of other fieldworkers. Perhaps it is even a *sine qua non* for fieldwork."

Some writers consider this tension to be a form of culture shock which Du Bois (1970) describes as a syndrome precipitated by the anxiety that results from losing all one's familiar cues; a syndrome which includes frustration, repressed or expressed aggression against the source of discomfort, an irrational fervour for the familiar and comforting, and disproportionate anger at trivial inferences. Fischer (1970:270) takes a similar line when she writes:

> "Symptoms include depression and concomitant inhibition of activities, rejection of members of the culture studied as beyond the pale in certain aspects, and paranoid feelings that one is being cheated, plotted against, or laughed at contemptuously."

Obviously psychological stress reactions (such as withdrawal, depression, open or suppressed hostility) are common and important to the performance and reporting of field research. Wintrob (1967) makes this very clear when he examines some of the available anthropological "self-reports" from a psychiatric point of view. Further he suggests that it is important for the field worker to understand his own psychological needs and responses, and to broaden his understanding of the psychological significance of the reactions of the people he sets out to study.

My own reaction to stress (related to feelings of inadequacy, fear of failure, real or imaginary rebuffal, at times ill health) was to withdraw. Previous research experience, and the first two months of this field period, when I was living in a camper on the caravan park, had taught me that I could function efficiently only if at least once a day I had an hour to myself when I could do what I wanted to do. Consequently, after I had established myself in a flat, I withdrew as a matter of course, every day, during the day, for about an hour in order to listen to music, read a novel, sleep. I found this essential in order to cope with the 12—14hours a day total involvement with the community. Another form of withdrawal occurred approximately every three months: when I left Rural Town altogether, I believe this constituted a more significant psychological stress reaction. The need to "escape" became very strong for the following reasons.

Data collection tended to evolve in cycles. For three or four weeks the days did not have enough hours for me to visit all the people who invited me, to record all the events and interactions open to me. This period of intense activity was invariably followed by "the doldrums" (lasting a week to 10 days) when I found it very difficult to make any progress, when I spent most of my time just "hanging about." Consequently this was a time of depression and anxiety. After "the doldrums" the active cycle recurred for another four weeks or so. As soon as I felt the influence of the next period of stagnation, I generally withdrew to Brisbane because I simply could not bear another ten days of frustration. After a week I returned to Rural Town and the Aboriginal people and I were ready for another cycle of activity. At the time I was unable to explain this pattern. In retrospect, I believe that it was due to two interrelated factors:

a. In our relationships Aboriginal people may have suffered as much tension as I did. To have an outsider there all the time asking questions, and feeling obliged to explain, dissect and interpret for this outsider must have tired people. So, in order to escape my expectations of them, they withdrew from the relationship; the distance gave everyone a chance to recuperate. Brigges (1970) found similar tensions and subsequent withdrawal among the Eskimo people with whom she worked.

b. I suspect that "the doldrums" marked periods when I was too full of impressions to absorb any more. Consequently I may have indicated this to people by subconscious rebuffs. If, as Du Bois (1970: 227) writes: "An eagerness to learn, as well as the conviction that what one is learning is important, has a way of communicating itself even when, or perhaps just because, it is not explicitly stated...." then the reverse must also be true. Being unable to absorb any more, I may have unconsciously transmitted this feeling of fatigue. Aboriginal people's withdrawal would then have been a reaction to their perception of my behaviour.

The effect of bias. But quite apart from the influence of stress and tension on the field worker and the coping mechanisms which may influence data collection and interpretation, ethnographers, as people, are subject to a number of biases which may also affect their descriptions significantly. There is no way to neutralize biases except, as Myrdal (1944: 1043) suggests, to face the valuations and to introduce them as explicitly stated, specific and sufficiently concretized value premises. Schwartz and Schwartz (1969: 103) also stress the importance of this approach and operationalize it by maintaining that any researcher must:

a. Be motivated to look for his biases.
b. Having discovered biases must explore their meaning and ramifications.
c. Consider the uncovering of his biases as a continuing of discovery.

I am aware that the following biases may have influenced my work:

a. I am biased positively towards Aboriginal people. This is not based on any "shame" or "guilt" complex but rather a result of a preponderance of positive interaction. During a seminar, an Aborigine once told me I saw "things too much from the Aboriginal point of view." I have never been sure whether to take this as a negative or positive comment.

b. During the early part of field work I felt very hostile towards people who, in my opinion, chastised their children too severely, or who did not give them the attention I considered necessary. I became more rational after long discussions with my chief assistant, Mrs. M. King. The early hostility and persistent concern may, however, have played a part in my later interest in socialization practices.

I am sure that there are many more biases related to my personality, my own German upbringing or my experiences as a migrant in Australia.

The influence of age, sex, and marital status. No matter how careful a researcher is to define his role or roles, his biases and the tensions under which he is working, it is impossible to escape the limitations dictated by age, sex and marital status. Mead (1970:320) stresses this when she points out that simply because every human society is composed of both sexes and a range of ages, the age and sex of a field worker becomes immediately important. Mead (1970) continues by pointing out that age, sex and marital status *determine* the kinds of data available to any ethnographer.

I am a woman and at the time of this research period (1974-1975) I was 28 and 29 years old, unmarried, and, more importantly, childless. This latter fact particularly singled me out from other women in this community where it is rare for a woman over twenty not to have borne at least one child. Further, I was alone, more educated than anyone else and I did, according to the people, quite unusual things for a woman, such as travelling on my own, writing reports, asking questions, interviewing, when I should have been concentrating my efforts on "getting a man." People continuously referred to my single state, worried about it, and told me that I would be lonely in my old age without a husband, and, especially, without children. My age, sex and marital status, then, restricted me to some extent, but they also proved advantageous in the development of some relationships.

Some very real difficulties were related, at least initially, to the fact that young women (aged between 17 and 20) tended to measure me as a possible sexual competitor. Golde (1970:87) found a similar reaction. I was able to cope with this aspect during the first three months of field work when I was being almost continuously "chaperoned" by my assistant Mrs. King, who is in her middle fifties. Because of her presence, her personality and age as well as my behaviour, I was able to establish a "good" reputation which is highly prized in this community. On the basis of this reputation (since I no longer posed a threat to anyone's husband) I was, unlike some other female anthropologists (see Mead 1970), permitted to take part in men's discussions and play a *passive* role in men's activities, such as drinking parties. The avuncular affection displayed by the oldest male in the community helped, as much as women's tolerance for my interaction with men to establish rapport with males of all ages in the Aboriginal group, provided I learned the rules of etiquette, and abided by them. No restrictions were placed on my interaction with men over fifty years of age. Younger men, however, were expected to associate with me only in public places, in the company of others, or in the presence of their wives. Nevertheless, while my age, sex and marital status imposed limitations on my interactions with men in the community, these factors also proved to be an advantage. Being a woman meant that the community as a whole felt, and acted in a very protective manner towards me. This protection took many forms—they worried about my health and sent someone around if I was missing for a day; they were concerned with my eating habits and continually asked me to come and eat with them because: "It's no good eating by yourself, I suppose you don't eat at all!" they felt that I might be lonely living by myself and asked me to stay in their houses, or at least have a young girl living with me (both offers I declined as I very soon had Aboriginal neighbours in the flat next to me); they warned me about hitch-hikers and the dangers of driving into the bush; the list is endless.

Being single and comparatively young also attracted the men who spent literally hours discussing politics, employment, alcohol, horses, cars and so on, with me. I believe that they enjoyed my company because I was different from their own women, because I wanted to talk to them and valued their opinions, because I had been to different places and possessed more general knowledge than other women and because I never judged their drinking habits as did other women. Men, then, treated me very chivalrously and I must admit that I relied strongly on this feeling in my data collection.

From the above discussion, it is evident that no researcher, as an instrument of data collection, can ever fulfil all the requirements of scientific inquiry. But perhaps the aim in participant observation is not value-free analysis but insight and understanding to the best capabilities of all involved. As Golde (1970: 11-12) writes:

> "That lesson, the learning of culture, is not an intellectually dispassionate one, nor is it even necessarily mediated by words, it is direct intuitive learning that seeps through all the senses, as it did when we were children being socialized into our own world. Fieldwork can be a replication, condensed in time, of childhood learning, with its attendant anxieties, mystifications, impotence, and occasional and gradual mastery. We may label this 'initiation anxiety', expecting to find perplexity, feelings of powerlessness, unsureness, and strain accompanying the process of being inducted into any ongoing, structed situation that is new . . . There is discomfort and anxiety in trying to balance a different way of seeing the world with one's own established perceptions . . ."

Keeping these reservations in mind, it is nevertheless possible and necessary to search for accuracy, precision and completeness of data (Zelditch, 1969), for the researcher is not only an individual but also a trained recorder.

The importance of recording. Two important considerations underlie the recording of observations and information in field work. Firstly, the ethnographer must continually check and recheck his data. As Pelto (1970: 92) stresses:

> "Field work requires much more than simply 'being there' passively watching what people are about. Often the fieldworker, in observing a particular pattern of behavior or an event, needs to find out a great deal more about that event than he is able to observe firsthand....relationships of this event to other types of data must be explored, and a host of other materials must be considered in order to make individual observations useful....he may need to devise ways of ensuring the representativeness and objectivity of his observations in a series of repetitions of a given event. By structuring observations and systematically exploring relationships among different events—through interviewing, watching, and perhaps administering 'tests'—participant observation can be converted to scientific use."

Koepping (1973) as well as McCall (1969: 130) maintain that a mental attitude of scepticism is basic to this process of check and counter-check. Koepping defines scepticism as a suspension of judgement and continues:

> "The cultivation of this kind of sceptical distance seems to provide us with a very useful attitude to combat not only the tendencies of dogmatism which develop so easily from fixed theoretical positions; the sceptical state of mind may help us to overcome the dangers of distortions due to disenchantment and alienation..." (Koepping, 1973: 52)

Secondly, the field worker must, at all times record *concrete* notes (Pelto, 1970). That is to say, the ethnographer must describe *the observations themselves* in great detail rather than the low-level inferences he derives from the observations. As Pelto (1970:93) points out:

> "Theoretical systems provide the concepts and . . . frame of reference for pigeon holing observations. The field worker needs to examine with great care the nature of the classification and concepts he employs in field observations. A constant danger is the overly quick leap to abstraction. Primary reporting of concrete events and things in field work should proceed at as low a level of abstraction as possible..."

The importance of such primary recording cannot be underestimated. Schwartz and Schwartz (1969) for example, (see also den Hollander 1967), clearly demonstrate that recording of observations can be influenced substantially by the time lapse between observing and recording. They write:

> "In retrospective observation the investigator re-creates, or attempts to re-create, the social field in his imagination, in all its dimensions, on a perceptual and feeling level... What occurs is a type of reworking of the representation of the phenomena as initially registered . . . Rather than finding a simple and direct connection between the occurrence of the event and its representation as data, we discovered that our observation began to expand the longer we thought about it..." (Schwartz and Schwartz, 1969: 92-93)

I suspect that a certain amount of retrospective reworking and abstracting goes on without the field worker's awareness; however in order to overcome these problems to some extent, I followed Professor Cawte's[5] suggestions and recorded information on two levels wherever I could:

(a) the actual, minutely described event

(b) my interpretation of the event

Cawte suggested a third level—the poeple's subjective feelings about the event—however it was not always possible to acquire these on the spot. People take time to consider situations and frequently their feelings about any particular occurrence could not be elicited until they wanted to discuss it.

Further, I recorded all interviews verbatim and all events or discussions on the spot or as soon as possible afterwards in order to minimize premature abstraction or other distortions.

The Informants

If the researcher himself (as an individual and as a 'recorder') may "contaminate" his data, then the material may also be distorted by his informants. The single, most frequently discussed question in relation to the effect of informants on data collection is: "Are they telling the truth?" This question, which any researcher must ask himself, goes far beyond the possibility that people are lying and opens up the whole debate of whether people are saying things which they feel the researcher wants to hear (see Langness, 1973). It seems impossible to me that deliberate lies (and I have never uncovered any in my own field work) can remain uncovered during long periods, of participant observation. The process of checking and rechecking information, the observation of interrelated events and so on, must effectively show the ethnographer if people are deliberately deceiving him. However, discrepancies often occur without any intent to deceive, simply because different people perceive events differently at any given time. As Dean and Whyte (1969: 105-106) point out:

> "The informant's statement represents merely the perception of the informant, filtered and modified by his cognitive and emotional reactions and reported through his personal verbal usages. Thus we acknowledge initially that we are getting merely the informant's picture of the world as he sees it."

Consequently, the central issue is not "how do I know this information is true," but rather, "what does this information tell me about the person and his feelings and perceptions." This realization still leaves us with the question of whether when people are telling us things, they are expressing attitudes and beliefs which they think we want to hear or which they feel are expected of them. I suppose this type of contamination may occur in any research and it may happen without the ethnographer's knowledge; I do not believe that it can persist over long periods of participant observation and day-today involvement in the community's activities.

The last major area in which data collection processes may be distorted during participation and observation is found in the interrelationship between researcher and informants.

Interaction of Researcher and Informants

Many aspects which may influence the interactions between ethnographer and the people with whom he is working have been discussed previously. These included tension, bias, definition of roles, the question of "truth" and so on. One central question remains and it is one which every anthropologist asks himself and which is constantly asked by others: "Were you accepted by the people?" Briggs (1970:20) confronts this issue after her field work among Eskimo people on the Black River. She writes:

> "People seem to expect a straightforward yes-or-no answer, but in fact there is no such easy response. What is meant by 'accepted'."(Briggs, 1970:20)

Further, Middleton (1973:228) warns anthropologists against fooling themselves into believing that they ever become complete members of the local community. I do not know how to define the concept of "acceptance" any better than does Briggs. Certainly I cannot claim to have become a full member of the Aboriginal community in Rural Town. I believe I was *tolerated;* I do not believe that all the people were happy about my presence and my questions all the time, but they put up with me. They tolerated me as a person, with all my *idiosyncracies;* they tolerated me as a researcher—a nosey, prying busybody. They reassured me, looked after me and worried about me. They are expecting me to maintain my ties with the community while I am away and they stress that I must return in order to fulfill my obligations to them. This process of *tolerance* and its related expectations is a mutual undertaking; without it, field work cannot proceed. I have, like Golde (1970: 92-93) learned that:

> "...the process of understanding another culture is the process of recognition; only by using the self as a filter, is it possible to weigh and evaluate differences and similarities. If at times I felt smug because of my education and training, habitual analytic reasoning, and ability to control my emotions, I also learned to accept an irrationality I shared with the people, to recognize my own susceptibility to social pressure and the need for the people's good will and affection to maintain my own feeling of security. Though I moved outside the system to a large extent, I was no less social a creature than anyone of them, depending on the day-to-day gratifications of human interchange for my own emotional sustenance..."

Obviously this kind of relationship can be developed and sustained only if it is mutually satisfying. The ethnographer's satisfaction is intrinsic in the enterprise and its resultant rewards. But the people must also see some compensation for their time and effort, and this compensation (material or nonmaterial) needs to be immediate. Possible long term effects of anthropological studies (such as perhaps a change in policy, aid in understanding, better welfare programmes) can never be guaranteed, and because of their futuristic, utopian nature, bring little reward to people in the present. The only possible way the field worker can justify his presence is to make sure that people are getting something out of him while the research is in progress. I have spoken to other ethnographers[6] who have complained that they spent too much time providing a taxi-service, or working in the community store when they should have been gathering data. Others have expressed resentment because people expected too many gifts or demanded materials which the researcher sorely needed himself. I have never experienced this kind of situation. Relationships between myself and Aboriginal people in Rural Town have always been based on *reciprocity*. Reciprocity implies a two-way process and, in my opinion, this mutual exchange of support, whether material or emotional, is basic to the interrelationship between the ethnographer and the people with whom he is working, as it is to any kind of interpersonal relationship. It seems to me that there is a great danger when field workers, in their early attempts to establish rapport, provide services which they cannot maintain throughout the project or when they are too inhibited to express any expectation of reciprocity. This can too easily lead to resentment when they become over-committed and feel "exploited" by the people. Further, the field worker will ultimately not fully understand the meaning of interrelationships in the community if he, himself, has not experienced the full, two-way process.

While the principle of reciprocity, in its fullest sense, ensures that the people with whom the anthropologist works are compensated in some way for his presence, it also safeguards the field worker's own self-esteem. He has no need to feel guilty about his demands on the community, nor will he feel "used" or "exploited." As Golde (1970) points out, reciprocity in some form can be the anthropologist's means of demonstrating her value, importance and membership in the community. She continues:

> "I demonstrated not only my usefulness, worth and importance, but also the fact that I could be counted on to keep up my end of the 'bargain'...For a few individuals, the nature of the return was more psychological than material: the prestige of friendship with me; knowledge of the world they might gain . . . and in a few cases, the freedom some felt to say things without censure of criticism, expecting an understanding and sympathetic listener . . . " (Golde, 1970:83)

Because of my own very limited resources, Aboriginal people in Rural Town never asked me for financial support. At times we mutually borrowed money from each other, but it was always repaid when finances improved. I did however became, and still remain, a major resource centre for the community. For example, I contributed by informing people about educational programmes, about job-training schemes, apprenticeships and housing programmes. I was asked to intervene when individuals did not receive prompt workers' compensation, when deserted wives and difficulties in obtaining Supporting Mother's Benefits, when there were problems with all forms of authority, when there was dissatisfaction with Government Departments and so on. At all times various educational, legal and housing services in the State supported my endeavours and without this support I could not have sustained credibility.

Like Golde (1970) I provided a great deal of emotional support to people in crisis. In their turn, the people looked after me, often fed me, sometimes bought me presents, and made me feel welcome, quite apart from the time and effort they invested in providing me with information.

Obviously this kind of interrelationship based on rapport, reciprocity and tolerance is a delicate arrangement. Like Nader (1970) and Fischer (1970) I was often doubtful whether or not some particular line of inquiry might not upset the rapport so important to effective field work. Fischer (1970), for example, who has experienced field work in various parts of the world and has worked among isolated traditional people in Truk, as well as a village in New England, maintains that this fear of offending sensibilities and invading privacy was strongest when she studied American culture.

Perhaps it is true that the field worker who is involved in a culture very much like his own feels the pressures of his own value structure more than one who is working in a totally alien environment. Because of this pressure and the real fear of offending others' sensibilities, my project may not fulfil Zelditch's (1969) requirements of efficiency—another ethnographer, with a different approach may have been able to accumulate the knowledge more quickly and consequently more efficiently in terms of cost per added input of information.

Because the reciprocal relationship is so basic to human interaction everywhere and because people often perceive the researcher as an "understanding and sympathetic listener," the field worker frequently finds himself in the position of counselor for people in crisis.

Prolonged Discussions with People in Crisis

When I speak of "people in crisis" I refer to all those individuals who were experiencing extreme tension and stress—often emotional as well as physical. Discussions with these people were always informal and in no way related to the general interviewing process. I rarely asked any questions, and I certainly never asked people to dissect and define their concepts. I was there, I listened, I recorded verbatim comments wherever possible, and I provided a shoulder to cry on, someone to whom the individual could unburden himself. In this manner it has been possible to truly record the "insider's" view of his situation. However, because I could not, in these times of extreme personal stress, ask people to provide me with detailed analyses of the concepts used, I have to analyse the resultant material, classify and categorise it from an "outsider's" point of view. This combination of "insider"/ "outsider" conceptualization is particularly difficult in crisis situations for a number of factors.

(a) At no other time, I believe, does the anthropologist become so emotionally involved in the actual situation
(b) At no other time is the informant so vulnerable
(c) At no other time does the researcher, who has no special medical/psychiatric training, feel so useless and parasitic.

The Use of Key Informants

Key informants are the life-blood of anthropological inquiry. They are indispensable for recording a people's history, that is, those events which have never been recorded in the past, as well as the people's subjective interpretation of recorded historical phenomena. Further, key-informants become central check-points in that they explain and possibly redefine information and observations. They occupy this central position in field work because they are, ideally, acquainted with and knowledgeable about their society to a larger extent than are other members of their group. Consequently their position needs careful examination. Key-informants, as individuals, must vary with regard to their knowledge and observational skills (see Young and Young, 1961). As a result, they should be carefully selected so that each sector, each sub-group in the community is fully represented by its most knowledgeable representative(s). Further, the ethnographer should progressively "train" his key-informants to make them more competent interpreters of their society. These two interrelated requirements are to some extent idealistic. I do not believe that any researcher ever selects key-informants purely because of methodological considerations. It is an ideal which he must continually consider; nevertheless interaction between key-informants and ethnographer will be sustained only if it proves congenial and satisfying for both parties.

Table 1 sets out my own key-informants by age and sex. Each family group is represented in this number as well as most age groups. However, I cannot claim that these people possessed more knowledge about their group, or that they were better interpreters of their socio-cultural system, than any other person of comparable age and sex.

Table 1

Key-informants in Rural Town by Age and Sex

AGE	SEX	
	MALES	FEMALES
15-20	2	1
20-30	1	3
30-40	2	3
40-50	1	1
50-60	2	1
60+	1	1
Total	9	10

They represent individuals who were interested in the research, with whom I could sustain a mutually rewarding relationship, and who were most cooperative in terms of time and effort. Dean, Eichhorn and Dean (1969) point out that luck plays an important part in the quality of key-informants selected. However, they continue, a researcher should seek out the following *kinds* of informants who will generally prove more helpful than the person selected by chance. These *kinds* of informants include, according to Dean, Eichhorn and Dean (1969:143)

1. Informants who are especially sensitive to the area of concern (the outsider, the "rookie", the nuveau statused and the naturally reflective and objective person).
2. The more-willing-to-reveal informant (the naive informant, the frustrated person, the "outs", the old hand, the needy person and the subordinate).
3. The critical cases (those with similar psychological and social characteristics as other members of their group placed in different sociological environments).

Obviously a combination of *types* may occur in any one individual key-informant and the element of luck cannot be eliminated. In my case, key-informants fall into the following categories—the naturally reflective and objective person, the frustrated, the old hand and the needy person. Critical cases came expecially to the fore during my analysis of drinking patterns and included many individuals who in the immediate past had experienced crisis or were in some way involved in the situation at the time.

These key-informants, then, aided in my understanding of the situation in Rural Town. While their comments provided checks on other people's accounts and my own data collection on the basis of participant observation, the reverse was also true.

It is very important to retain a "sceptical attitude" (Koepping, 1973) perhaps especially in relation to key-informants because they are not always "scientifically" selected and because of their central interpretive role. As Pelto (1970: 96) points out:

> "...in some studies which have involved great dependence on key informants over long periods of time, the possibility cannot be ruled out that the interpretations of cognitive structuring, basic postulates, or functional relationships which appear to emerge naturally from the key informants were in fact shaped to a considerable extent by the anthropologist himself as he taught the key informant his role as expositor of a cultural system."

At all times, then, key-informant interviewing was closely integrated with participant observation where key-informant data is based on majority agreement among a majority of informants.

Throughout this project the process of meeting people and establishing rapport as well as checking and re-evaluating data was simplified by the work of Aboriginal assistants.

The Role of Aboriginal Assistants. I have employed Aboriginal assistants since my first field experiences during 1969 and I continued the practice in this project. I was fortunate in that I could engage the services of Mrs. M. King for the first three months of my work in Rural Town. I had worked with Mrs. King extensively since 1969 and although she did not belong to the Rural Town community, she had many kin ties with the group. In many ways then Mrs. King was an "outsider", but she was also an "insider" by virtue of her Aboriginal descent and kin ties. She proved an invaluable asset as a liaison between Aboriginal people and myself, in teaching me new methods of approach and in discussing at great length all events and comments we had observed. She was also instrumental in relieving much of the stress and tension associated with early field work.

After Mrs. King left me to join another research programme, I employed two women, who had previously been key-informants, aged between thirty and forty, in a similar capacity at various times. Although they lacked many of Mrs. King's skills acquired during a number of years of experience, they did spend many hours in extending and strengthening my network of informants as well as in discussing all aspects of the work. The role of "assistant" was alien to these women, who were quite prepared to exert effort and time on my project, but felt very reluctant to accept payment for the work. I insisted on this because I felt that the small remuneration was really a very insignificant reimbursement for the amount of involvement I expected, and received. The work of my assistants, then, was different from that associated with key-informants in that it involved eight to ten hours a day of walking around with me, discussing various topics with other members of the Aboriginal community and then rethinking and re-evaluating everything we had seen. At all times we worked as a team. I never asked my assistants to collect independent data, because I defined their main tasks as teaching me to understand as much as acquiring new information. Further, no assistants were involved in the actual interviewing process because of the problematic nature of this method, and because there was neither time nor money to train them fully in the technique.

There are a number of reasons why I have found assistants of great value in my work:

(a) I believe that it is possible to establish rapport only through a system of referral and assistants play a major part in making such a system accessible to the field worker.

(b) I believe that Aboriginal people will generally feel more relaxed and at ease when approached in the company of one of their own people.

(c) I know that the presence of an assistant relaxes me and enables me to make the best possible use of new contacts.

(d) I know that the support of an assistant relieves the stress of field work.

(e) I believe that the progressive training of assistants is part of the reciprocal relationship involved in field work. Assistants acquire new skills, a good deal of ego satisfaction and through them "anthorpology" becomes a more meaningful concept to other Aboriginal people.

(f) I believe that any person who is associated with my projects, at the level of intensity I expect from my assistants, should be remunerated at an acceptable level.

In this analysis of my methodology I have attempted to guide my reader through the steps involved in establishing my methodological/theoretical approach. I have defined my tools of research and the problematic nature of these techniques and I have tried to analyse some of the limitations associated with the research enterprise. The problems of validity and reliability remain and may best be summed up by den Hollander's (1967: 32) statement:

> "Though, scientifically, there may be many objections to the wholistic social description of local communities and though perhaps it will never be capable of being carried out in a purely scientific fashion, we would yet reluctantly do without it. It remains one of the more attractive activities in the field of Social Science or, let us say, in the para-scientific field. Its attraction is, in fact, partly due to the fact that excellence here cannot be achieved exclusively through proficiency and experience of the craftsman but is still closely dependent on that very personal factor, the semi-artistic one, which is subject to so much censure....However profitable it may be to reflect on the weaknesses of social description it would be unfair and even harmful to make perfectionist demands where perfection cannot be attained."

REFERENCE NOTES

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[2] The term *Aboriginal* is applied throughout this paper to all individuals who identify as such, no matter what their admixture of ancestry may be.

[3] Mardil—the term by which Aboriginies in this area identify themselves.

[4] Mate—Australian colloquial for friend.

[5] Private discussion 1974.

[6] A.I.A.S. Biennial Conference 1974, Problems in the Field. Inability to cope with the stress and anxiety associated with field work may have influenced researchers' reactions to and perception of peoples' demands.

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CROSS-CULTURAL COMPARISONS OF ANXIETY: METHODOLOGICAL PROBLEMS

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Introduction

Anxiety is a central explanatory concept in almost all contemporary theories of personality. According to Lewis (1970), there has been an increase in research on anxiety from 3 articles in 1927 to over 200 in 1966. Since 1950, 5000 articles and books on anxiety have been published (Spielberger, 1972a) with diverse attempts at comprehensive theories and differing methodological formulations. Nevertheless, when it comes to definition, the concept of anxiety is still ambiguous. Indeed, the title of Lewis' (1970) paper is "The ambiguous word 'anxiety'." At various times, anxiety has been conceptualized as a response, a stimulus, a trait, a motive and a drive (Spielberger, 1972a, 1972b). In a critical analysis of various conceptual models of anxiety developed in the United States, Spielberger (1972b) points out that most investigators have defined anxiety in terms of complex personality processes with multiple components and each investigator has tended to include different aspects or components in his definition of the anxiety process. Even when investigators include the same components, they often use different descriptive terms in defining these variables. This means that standard terminological conventions are not yet established for describing critical variables involved in the anxiety phenomenon. In addition, Spielberger (1972ab) has argued that ambiguity and semantic confusion also arise from the more or less indiscriminate use of this term "anxiety" to refer to two very different types of concepts. Recent findings suggest that it is meaningful to distinguish between anxiety as a transitory emotional state (State Anxiety or A-State) and a relatively stable personality trait (Trait Anxiety or A-Trait). The trait-state distinction was initially formulated by Cattell and Scheier (1961) and has been emphasized by Speilberger (1972a, 1972b). Further, Sarason (1972) has stressed the importance of situational, cognitive and perceptual factors in anxiety research, pointing out that much research is needed to fill the gap in present knowledge. There seems to be general consensus only on two issues, namely (a) stress produces anxiety and (b) cognitive factors (cognitive appraisal and re-appraisal) play a critical role in the arousal of emotional states.

The foregoing discussion shows that a great diversity of theoretical orientations guiding anxiety research exists even within the cultural context of the United States. When such divergent theoretical models of anxiety are applied and investigated in cultures other than the United States, the confusion *appears* to increase to a chaotic level. In *reality*, such research has its own advantages. For instance, cross-cultural research

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can extend the range of exact content of independent variables involved in anxiety beyond that found in American culture and can make anxiety theory more precise by identifying cultures in which people seem to behave out of line with the predictions based on an anxiety model developed from observations in the United States only. These and other potential contributions of cross-cultural research of this sort have been discussed in detail by Triandis (1972), Brislin, Lonner and Thorndike (1973) and Brislin (1976).

Based on different theoretical formulations of anxiety, a number of psychometric instruments have been developed to assess individual differences in general and test anxiety in adults and children. Spielberger (1976) has briefly discussed these various self-report measures of anxiety. In current cross-cultural research, the Cattell and Scheier (1963) IPAI Anxiety Scale and A-Trait and A-State scales of State-Trait Anxiety Inventory (STAI), developed by Spielberger, Gorsuch and Lushene (1970), have been used. In his book, *The Psychology of Anxiety*, Levitt (1967) reviews and evaluates a number of different anxiety inventories. He concludes that: "The STAI is the most carefully developed instrument, from both theoretical and methodological standpoints from those presented in this cahpter. The test construction procedures described by Speilberger and Gorsuch are highly sophisticated and rigorous". The STAI (A-Trait) is highly correlated with the IPAI Anxiety Scale, the Taylor Manifest Anxiety Scale, and appears to measure anxiety proneness in social evaluation situations (Spielberger et al., 1970). An important characteristic of the STAI scales is the brevity of administration. This scale has been used extensively in the United States for research, as well as for counseling and clinical purposes. It is indeed impressive that only six years after publication, more than 580 studies on STAI have been completed. Moreover, the STAI has been carefully translated into Spanish, Hindi, French, Italian, Swedish and Turkish languages. In addition, preliminary translations of the STAI are available in Danish, German, Greek, Hebrew, Hungarian, Japanese, Malay, Norwegian, Polish, Rumanian, Russian, Slavic, Vietnamese, Luganda and Swahili (Spielberger and Diaz-Guerrero, 1976). Thus, sufficiently well-standardized versions of the STAI exist in different languages to permit a great deal of genuine cross-cultural research on self-report anxiety. Of course, the STAI suffers from the general limitations of being a self-report measure. For instance, Smith (1974) showed that STAI scores are quite susceptible to intentions to exaggerate stress effects, just as these scores are readily influenced by other response sets (Allen, 1970). In spite of some shortcomings, the English STAI, and its carefully developed versions in different languages, represent an encouraging development for the assessment of trait and state anxiety in different national/cultural groups. Using the STAI, investigators from different countries can compare their findings with relatively greater confidence. Given the unique qualities of anxiety as an emotional state and the fact that eveyone experiences the emotion from time to time, *genuine* cross-cultural research on anxiety would seem to prove a promising approach for establishing general laws about human experience.

The goals of this paper are to (1) provide a brief review of major cross-cultural studies comparing anxiety *levels* across different cultures using self-report anxiety measures, and (2) to suggest an alternative strategy for developing tools for cross-cultural research on anxiety.

Major Cross-Cultural Comparisons of Anxiety Levels

The studies reviewed in this section have been organized in terms of the anxiety tools used for cross-cultural comparisons.

Studies on IPAI Anxiety Scale

In the early sixties, Cattell and associates compared anxiety levels of college students across different cultures. They preferred to call these studies cross-national comparisons because they argued that nations are the only easily definable units of study and these units don't correspond one-to-one with cultures. In one study, Cattell and Scheier (1961) compared six nations: United States, Britain, France, Italy, Poland and India. The sample size for each nation was quite large (91 to 422 college students) and matching was reasonably good from nation to nation as to sex composition, age, education, etc. The estimate of anxiety level was based on the twelve item total for combined factors 0+ Q^4+ and C- derived from 16PF items which were available in the primary language of six nations. On the 16PF, the 0M factor refers to degree of guilt proneness, the LQ^4M factor denotes the degree of ergic tension (e.e., stimulation, satisfaction), and the C- factor indicates the degree of ego strength. Thus estimates of anxiety were not as broadly based as one would wish. The data showed the following order from high to low anxiety: Poland, India, France, Italy, Britain and the United States. Jalota (1957) had also reported that Indian university students are more anxious than their American university counterparts. But, in another study, Cattell and Warburton (1961) found that American college students had a somewhat high level fo anxiety than the British College students. This reserves the British-US trend reported in Cattell and Scheier's (1961) study. It may, however, be noted that a larger pool of items was used in Cattell and Warburton's (1961) study. When the differences were examined in terms of primary personality dimensions, it was hypothesized that higher American anxiety arises from greater ergic tension (excess of stimulation, oversatisfaction) and greater guilt proneness. Thus, the results have been discussed in terms of both *levels* and *patterns*. Tsujioka and Cattell (1965) made extensive Japanese and American comparisons using a sample of 300 Japanese male and 117 American male undergraduates. Highly significant differences appeared in the direction of the Americans who were lower in anxiety levels than the Japanese. McQuaid (1967) applied American norms of 16PF to eight samples of Scottish subjects, partially confirming Cattell and Warburton's finding. No mention was made concerning the equivalence of item meanings for these samples

Studies on the STAI

In the early seventies, a number of cross-cultural comparisons of anxiety levels were made utilizing the English STAI and its carefully developed versions in different languages.

Speilberger, Sharma and Singh (1973) administered the A-State scale of the Hindi STAI to a sample of 92 graduate students in India under standard (NORMAL) instructions, and then with instructions to respond according to how they believed they would feel just prior to the final examination in an important course (EXAM Condition). These data were compared with those reported for American college students (Spielberger et al., 1970). There was a substantially greater increase in STAI A-State scores for Indian graduate students than for their American counterparts. In addition to other variables, this difference seemed to reflect the differences in the examination systems in the two cultures. In general, Indian students are examined less frequently than American students and it appears that they perceive examinations as more stressful. In another study, Le Compte and Onner (1976) compared the published normative data on the STAI for Turkish (N=499), American (N=1800), Puerto Rican bilingual (N=481) and Hindi samples (N=160). The order of A-Trait mean scores for the four countries from low to high was Puerto Rican bilinguals, American, Hindi and Turkish. The corresponding order for A-State mean scores, from low to high, was Hindi, American, and then a tie between Turkish and Puerto Rican born samples. Pancheri, Bernabei, Bellaterra and Takaglione (in Spielberger, 1976) compared STAI A-Trait means for Italian and American male and female college students. Italian students were significantly higher on trait anxiety than their counterparts in the United States — the effect was especially pronounced for Italian-US females in contrast to Italian-US males. In another study, Biaggio, Natalicio and Spielberger (1976) reported that A-Trait means were *slightly higher* for Brazilian male and female students than for US students as was the A-State mean score for Brazilian females (Spielberger et al., 1970). In contrast the mean A-State score for the Brazilian male was *slightly lower* than the corresponding score for US college freshmen. Such slight dissimilarity in responses to the STAI can also be accounted for by the subtle irregularity in translation of the STAI from English to Portuguese. Moreover, the number of males in the Brazilian sample was too small (N=18) to have any interpretive significance.

In all four of these studies involving the STAI and its well-calibrated versions, the mean anxiety scores of cultures other than the American were compared with normative data reported in the Test Manual of the English STAI (Spielberger et al., 1970). This means cross-cultural comparisons have been made before normative data for new cultures were developed. A norm based on one culture is probably *not* applicable to another culture. Therefore, it's necessary "to develop normative data for a culture before cross-cultural comparisons can be made" (Wesley & Karr, 1966). Hence, all these comparisons of anxiety levels across cultures are weak.

In two other studies with the STAI, the anxiety measures in both the cultures were taken by the investigators themselves. In one of these studies, Lugo-Quinones (1975) compared Anglos, Mexican-American, and Puerto Rican sophomore students in regard to state and trait anxiety. The Anglos and Mexican-Americans were given the English STAI, while Puerto Ricans were administered the Spanish version of the STAI (Spielberger, Gonzalez, Martinez, Natalicio and Natalicio, 1971). Comparisons of the raw scores revealed the following position from high to low in anxiety levels. State Anxiety: Puerto Ricans, Anglos, Mexican Americans. Trait Anxiety: Puerto Ricans, Anglos, Mexican Americans. Thus the order is similar for both trait and state anxiety, with the Mexican-Americans the lowest in regard to both trait and state anxiety. In another study, Endler and Magnusson (1976) compared trait and state anxiety scores of male and female undergraduate students of Stockholm, Sweden with their counterparts at York University, Toronto. Besides the STAI A-Trait scale, a revised form of the SR Inventory of General Trait Anxiousness (GTA) (Endler & Okada, 1975) was used to measure trait anxiety. Similarly, in addition to the A-State scale of the STAI, a modified form of Behavioral Reactions Questionnaire (BRQ) (Hoy and Endler, 1969) was administered. With respect to cultural differences, Swedish males and females combined reported more physical danger and ambiguous (novel) trait anxiety and less STAI A-Trait anxiety than Canadian males and females combined. With respect to sex differences, Swedish and Canadian females reported more anxiety on the STAI A-Trait scale and for the physical danger situation than Swedish and Canadian males combined. However, Swedish and Canadian males combined reported more anxiety for daily routines than Swedish and Canadian females combined. Further, no significant cultural differences were found for both state anxiety measures, i.e., STAI A-State scale and BRQ.

In all the studies involving the STAI, the state anxiety scores of subjects across different cultures have also been compared. This is an unsound comparison. It needs to be reemphasized here that A-State scores reflect transitory emotional states that are experienced by an individual at a *particular moment* in time. Further, these states *vary* in intensity and duration and *fluctuate* over time as a function of both the amount of stress that impinges upon an individual and that individual's interpretation of the stressful situation as personally threatening. Until and unless the stresses (objective stimulus properties of a situation) and their subsequent cognitive appraisal by different cultural groups at the time of test administration can be empirically shown to be comparable, such comparisons can be misleading.

Studies on TASC

In two studies, the Test Anxiety Scale for Children (TASC) (Sarason, Davidson, Lighthall, Waite, Ruebush, 1960) has been employed. In one of these, Holzman, Diaz-Guerrero, Swartz, Tapia, Laosa, Morales, Lagunes & Witzke (1975) conducted a longitudinal study of personality development in Mexico and the United States. Besides the TASC, the Holtzman Inkblot Test (Holtzman, Thorpe, Swartz & Heron, 1961) was also used. The one variable of interest here that is measured by Holtzman's test is anxiety in fantasy (Ax). The sample consisted of 900 children, distributed in six groups of approximately 150 subjects of three different ages in the two cultures. The major finding was that while Mexican children showed greater test anxiety, American children obtained higher scores on Symbolic anxiety as measured by the Ax than did Mexican children. Diaz-Guerrero (1976), in another study of 1st, 4th and 7th grade children, confirmed the findings of Holtzman et al.,(1975). In addition, he found that regardless of culture, subjects from the lower SES scored higher on the test anxiety scale than those from the upper SES. Further, regardless of culture, girls scored higher on the test anxiety scale. Some of these differences may be due to greater defensiveness and a tendency to fake good by the boys, as indicated by their higher scores on Lie and Defensiveness scales.

Methodological Problems

A perusal of various studies outlined earlier shows that there are *apparent* differences in mean anxiety scores (both general and test anxiety) among college and school samples belonging to different cultural/national groups. In *reality*, these differences may be artifactual and the interpretations faulty and premature. This may also be the byproduct of certain methodological limitations or errors. In this section some of the methodological and related problems will be discussed.

Problems of Translation

One important source of error can be the inadequacy of the translation of a psychological scale. The task of translation which *appears* to be simple turns out to be far more difficult than most people realize. Brislin (1970) and Brislin, Lonner and Thorndike, (1973) have discussed the various translation strategies and associated problems in a general context. When dealing with highly sensitive, subjective inner feelings assessed by the STAI the problems are confounded still further. Spielberger and Sharma (1976) have discussed the idiosyncratic problems that were faced in translating the English STAI into different language systems. In addition, Sechrest, Fay and Zaidi (1972) have provided a thoughtful analysis of problems of equivalence in translation. They have discussed this issue in terms of vocabulary, idiomatic, grammatical-syntactical, experiential and conceptual equivalence. In addition they refer to what they call "the paradox of equivalence". This paradox is that if one demands that a form of a test yield comparable results in two cultures in order to demonstrate equivalence, then the more equivalent two forms become, the less probability of finding cultural differences. On the other hand, if one looks predominantly for cultural differences and ignores the problem of equivalence, the greater the probability of finding cultural differences. In developing various versions of the English STAI, the criteria of cross-language equivalence were comparability in terms of means, psychometric characteristics, and high correlations between the original language version of the scale and its translation. Following the logic offered by Sechrest et al. (1972) it is possible that the cultural differences reported in these STAI studies may, in reality, be somewhat less than the real differences. While the resolution of such an equivalence paradox is not simple and no concrete solutions are yet available, it is clear that out of various self-report measures of anxiety, the STAI has the distinction of having been carefully translated in 20 languages, following both a more or less common theme and a similar sequence of development. Commenting on the STAI, Holtzman (1976, p. 177) observed, "few indeed are the verbally based psychological techniques that have been handled as rigorously in cross-cultural, cross-language research." Thus, translation inadequacies are far less in the case of the STAI than other self-report measures of anxiety. But the outer boundaries of cultural adaptation of the STAI are not known. Obtaining semantic equivalence among Western language versions and tonal languages of South East Asia or non-literate tribal languages of various parts of the world could well prove to be a most arduous task (Holtzman, 1976, p. 81).

Problems of Interpretation

In various studies dealing with cross-cultural comparisons of anxiety levels, both culture and language differences are confounded. But assuming the close equivalence of different language versions (as can be claimed by the STAI) comparisons may be interpreted purely in terms of culture. Still, these comparisons are rather weak because the cultural differences in test-taking attitudes such as response set, acquiescence and willingness to admit anxiety, etc., are also confounded. Until much more is known about the situational and methodological factors that influence the reaction to items in a self-report inventory, it is impossible to draw strong conclusions. Further, on the basis of a single self-report measure of anxiety, for which the item content is focused on symptoms or feelings (e.g., IPAI Anxiety Scale or STAI), it is not possible to provide a coherent interpretation of these differences in anxiety *levels*. Systematic studies of cross-cultural differences in anxiety *patterns* could be helpful in the interpretation of data, but few efforts have been made in this direction. There is apparent lack of comparability of samples on some variables and this too makes the validity of the comparisons dubious. Researchers should describe at least the following characteristics of their samples (Brislin and Baumgardner, 1971): age, sex, educational level, income, occupation, special training, and place of residence in relation to other members of the culture. In short, the description should include all the characteristics of the subject and the environment which could potentially influence the results or their interpretations. Moreover, the interpretations of many obtained cultural differences in anxiety are speculative and *post hoc*. Thus not much in the way of cross-cultural implications are apparent.

We are still not sure of the number and nature of dimensions of anxiety and their relative importance in different cultures. Frijda and Jahoda (1966) have suggested that, "Cross-cultural comparisons may have to be supplemented by cross-cultural comparisons on other dimensions to rule out obvious alternative interpretations of results (p. 123)." Various studies mentioned earlier, by and large, failed to account for alternate interpretations of their data. Only one study by Diaz-Guerrero (1976) administered a Lie scale and a Defensiveness scale along with a test anxiety measure to rule out the role of these factors in contributing to anxiety differences. Campbell (1969) has suggested a general approach to solving such cross-cultural methodological problems with what he calls the "*plausible rival hypothesis approach.*" He suggests that during all stages of investigation, the researcher list the potential reasons that could explain his data in addition to the preferred hypothesis he is actually testing. Alternative plausible hypotheses might be sampling problems, translation inadequacy or another cultural difference besides the researcher's preferred interpretation. The preferred interpretation is usually a theory based concept meant to explain cross-cultural differences, while the plausible alternatives are most often methodological difficulties such as sampling of subjects (Brislin & Baumgardner, 1971), test taking attitudes, experimenter and demand effects, and representativeness of the experimental task in relation to everyday tasks with which subjects are familiar. In addition, the researchers from each country should independently analyze the data so that estimates of inter-interpreter reliability can be made.

Situational Variables

The importance of anxiety-producing situations for anxiety research has been stressed by Endler and associates (e.g., Endler & Okada, 1974, 1975; Endler and Magnusson, 1976). Various studies have shown that college students respond differentially to four situations: the physical danger situation, the ambiguous situation, the interpersonal ego-threat situation and daily routines situation within a culture and among different cultures. The omnibus measures of general anxiety overlook such findings. Thereby a comprehensive, multifaceted view is ignored in preference to an oversimplified narrow concept of anxiety in cross-cultural research. A more fruitful approach might be to first intuitively identify plausible anxiety-arousing situations within a culture, then to find out empirically whether they are so, and finally, to vary them within a given culture. An excellent place to start such a research program would be within the educational institutions in which testing is done. If students in India feel more anxious within the school setting than respondents from other nations/cultures do, the elevations of the A-State scores on the Hindi STAI scale might be a predictable consequence. Cattell and Scheier's (1961) suggestion of searching for cultural *conditions* associated with differences in anxiety is also worth a serious followup. They suggested that we treat nations as individuals, correlating a comprehensive sample of national characteristics over a range of nations. This will yield correlations between national characteristics for the samples of nations studied. Such a correlation matrix can be factor analyzed to discover along which nations differ from one another.

An Alternative Strategy

In various studies reported in this paper, the measuring instruments were first conceived and developed in American settings. Later on, the tools were translated and administered to samples of other cultures with a goal to vindicate a conceptual model of anxiety developed in the United States by attempting to show that it yields results in a variety of cultures. Wober (1969) has called this a "*centri-cultural*" approach. In such an approach there is a danger of imposing (consciously or unconsciously) the psychological values of one culture on another. Discussing the same issue, Serpell (1976, p.54) comments, "If theories originally formulated to explain the behaviour of Western populations are to be productive in cross-cultural research they will need to be prised free of particular standardized tests, and new instruments devised appropriate for measuring the same psychological construct in different settings." The faulty centri-cultural approach was questioned by Berry (1969) and he proposed an alternative procedure, based on the emic-etic distinction, for developing and validating instruments specifically designed for cross-cultural use.

"The emic-etic distinction is now one of the central concepts in current thinking about cross-cultural research" (Brislin 1976, p. 217). According to Brislin (1976), this

> Distinction relates to two goals of cross-cultural research. One is to document valid principles that describe behaviour [experiences] in *any one* culture under study, taking into account what the people themselves value as meaningful and important [an *emic* analysis]. The second goal is to make generalizations across cultures that take into account *all* human behavior [experience (an *etic* analysis)] (p. 217).

Various research methods meant to yield both *emic* and *etic* findings have been evolved and evaluated (Brislin, 1976). For instance, Przeworski and Teune (1970) "suggested that a core of items be written that are meaningful and hence answerable by members of each culture. These would be items relevant to all cultures [(core items)]. In addition, *culture specific items* would be written that are designed. . . for each of the cultures under study (Brislin, 1976, p. 219)." The purpose of these items will be to show aspects of each culture that are not meaningful to the others. Interpretable statistical inter[cor]relations would be expected between culture general and culture specific items (Brislin, 1976, p. 219)." Brislin (1976) has provided a detailed discussion of the interpretations of such inter-correlations. The discovery of a set of core items and culture specific items will pose a difficult problem. One way would be to engage in collaborative research in which investigators will benefit from their observations of their respective cultures. In addition, the indigenous work along the lines of the Durojaiye (1975) study can be helpful. Durojaiye (1975) has studied the patterns of anxiety among Ugandan adolescents. He selected 29 items representing the most common worries from a pretest, presented these to another comparable sample and then factor analyzed the data. Six factors emerged from the analysis depicting distinct patterns of anxiety. He suggested that these distinct patterns may lead to further hypotheses useful in creating the criteria to decide which variables to include in any future construction of an anxiety scale for African adolescent and school pupils. All such analysis can also help in developing anxiety scales specially designed for cross-cultural use. So far no such attempt has been made.

To conclude, it is hoped that the use of such anxiety scales specifically designed for cross-cultural use will lead to *real* cross-cultural research by providing an "olympian view of cross-cultural data" (Berrien, 1967, p. 44). Such research will then provide answers to questions such as: Why are there cultural differences on some anxiety scales and not on others? Do the present cross-cultural anxiety results hold for populations other than school or college samples? Until the fundamental issues raised in this paper are properly resolved, cross-cultural comparisons of anxiety cannot reach the high order of scientific rigor and definition that must eventually be achieved if this interesting area of psychological inquiry is to make a truly significant contribution to our understanding of human behavior and experience.

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